Psychological Bulletin

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Psychological Bulletin

RESEARCH ON SENSORY INTERACTION IN THE SOVIET UNION[1]

IVAN D. LONDON
Brooklyn College

Since the early thirties the general area of sensory interaction has been the object of considerable attention by researchers in the Soviet Union.[2] However, owing to the language barrier and the near unavailability of the literature, most of the experimental and theoretical work that has been done in this field has gone unnoticed. Perhaps another factor tending to keep western attention averted has been the discouragement induced by the difficulty encountered in attempting to duplicate the reported findings.

It is true that much of the Soviet work on sensory interaction adheres to standards of execution, reportage, and interpretation that would be quite unacceptable to the western researcher. As a matter of fact, even a casual survey of the Soviet literature yields ready evidence of inadequate instrumentation and methodology, scanty detail, and a primitiveness in the statistical treatment of data which makes anything beyond an arithmetic mean a rare encounter.[3]

Nevertheless, western work on sensory interaction has been, in the main, scattered and desultory, whereas in the Soviet Union the subject has been given systematic and sustained attention (**172**, p. 77). For this reason alone a survey of the claimed experimental findings and of the correlative theorization seems to be in order.

The writer proposes first to outline representative basic findings, then to discuss the conditions that are said to affect interaction among the sense organs, and finally to sketch the theory that has been developed to account for this interaction. By

[1] Writing of this paper was initiated in 1952 in the course of association with the Harvard Project on the Soviet Social System, AF No. 33 (038) 12909, Russian Research Center, Harvard University. The writer gratefully acknowledges the subsequent support of the Russian Research Center.

[2] Some western students of Soviet psychology have seen in this the hand of Marxist ideology (**13**, pp. 161–162). The real reasons for the concentration of research in this area, however, are probably more on the pedestrian side. The writer discusses them in a paper to be published elsewhere.

[3] The published reports and resolutions of various professional meetings (not to be confused with the stage-managed affairs of the Lysenko variety), along with the abstracts of the discussions following the reading of papers, provide a frank admission of some of these deficiencies (**5**; **15**, p. 805; **59**; **109**; **116–118**; **172**, p. 55; **198**, p. 387; **347**; **382**; **484**; **485**; **486**, p. 249; **487**, p. 157; **488**, p. 623; **489**, pp. 650–651; **492**, p. 715; **495**, p. 330). Recently standard deviations and coefficients of correlation have been making a spotty reappearance after their exit from the psychological scene in 1936. Tests of significance are still only occasionally encountered (**13**; **88**; **197**, p. 45; **257**, pp. 79–80; **335**; **339**, p. 402).

research in sensory interaction[4] will be meant generally investigations that explore modifications of response in one sense organ under direct stimulation when another sense organ has been or is subject to its own characteristic stimulus. The latter stimulus is referred to in the Soviet literature as "indirect" (*nepriamyĭ*) or "accessory" (*pobochnyĭ*)—the last being the preferred term in this paper. Excluded for the most part from the survey will be intersensory research on (*a*) sense organs in pathological states (**12, 75, 77, 153, 196, 475–477, 479**); (*b*) nonsensory reactions (electrical and ionic excepted) of the sense organs;[5] (*c*) binaural interaction (**1, 2, 197**); (*d*) binocular and other intramodal interaction (pertinent exceptions being made, however);[6] (*e*) the effect of pain, as the accessory stimulus, on sensory response (**375, 393, 474**); and (*f*) the influence of interoceptive stimulation and dermal irradiation (**62, 63, 135, 162, 179, 185, 187, 191, 314, 315, 317, 321, 355**). The writer wishes it to be understood that all statements of ostensible fact in the paper refer to data or interpretation of data generally accepted as valid in the Soviet literature. The survey covers the period from 1930 to the present.

Changes in Sensory Response under Accessory Stimulation

Absolute Sensitivity of Peripheral Vision

The absolute sensitivity of peripheral vision was early shown to undergo modification with excitation of the nonvisual organs (**34**). Modifiability of peripheral sensitivity was demonstrated also with excitation of the central visual system which, together with the peripheral system, constitutes the visual apparatus. Thus, illumination of the macular field was found to reduce sensitivity of the peripherally placed rods shortly after cessation of the stimulus (**68, 73, 75, 93, 172, 286, 294, 326, 402, 403**).

The antagonistic relationship surmised to obtain between the central and peripheral retina has been the subject of considerable study with results confirming and extending earlier findings (**325, 329, 357–359, 403, 480**). For example, it was noted that illumination of the central retina has no effect on the retinal periphery in the color blind, whereas, in those whose cone apparatus is nondefective, characteristic modifications of peripheral sensitivity are to be observed (**265**, p. 30; **357**; **358**). Thus, the reciprocal nature of the cone-rod relationship, which Orbeli had earlier hypothesized, came to be an accepted fact (**69**; **233**, p. 473; **256**, p. 47; **285**; **371**; **436–438**).

While macular illumination, after its cessation, induces a reduction in peripheral sensitivity, a subsequent increase was found. Thus, after macular illumination with moderately bright light for one to ten minutes, a decrease in peripheral sensitivity for the first few minutes is exhibited, followed, however, by a period of supernormality (**286–288, 400, 401**).[7]

[4] Literally, interaction of the sense organs (*vzaimodeĭstvie organov chuvstv*).

[5] E.g., modifications of intraocular pressure, angioscotomata, etc. (**384–388, 475–478**).

[6] E.g., binocular contrast, various processes of retinal induction and sensibilization, simultaneous olfactory stimulations, etc. (**3**; **43–47**; **48**, pp. 209–215; **49**; **53**; **54**; **111**; **113**; **128**; **194**; **195**; **274**, pp. 258–259; **451**; **481**).

[7] The first phase is considered as an instance of inhibition; the second, as one of disinhibition. However, "sensibilization" is not considered to be derived always from disinhibitory processes. Thus, the sensitizing action of threshold stimuli upon peripheral vision is ascribed to the increase of neural excitability caused by weak excitation of the visual apparatus (**274**, pp. 129–130, 185–186).

Not only does macular illumination of one eye effect a reduction in the sensitivity of its retinal periphery, but a diminution is to be observed likewise in the companion eye. Further study, however, revealed that more or less prolonged adaptation of one eye to moderately bright light results in a heightened peripheral sensitivity in the other, while increased central sensitivity is in turn reported subsequent to peripheral illumination. It was also established that preliminary adaptation of one eye to red light of low intensity heightens very markedly the peripheral sensitivity of both eyes (**85–88**; **117**; **120**; **143**; **209**; **216–218**; **274**, pp. 182–186; **286**; **288**; **289**; **291**; **292**; **400**; **402**; **444**; **472**; **475**).

Pronounced effects on the peripheral sensitivity of the visual organ have been demonstrated as a consequence of auditory stimulation (**32**, **78**, **82**, **137**, **161**, **178**, **180**, **183**, **342–344**, **345**, **405**, **446**). For example, it has been shown that peripheral sensitivity, as a rule, declines on exposure to sounds of average or above average intensity. According to report, the noise of an airplane motor actually drops peripheral sensitivity to as low as 20 per cent of the level obtaining under conditions of quiet (**32**). However, a brief period of hyperventilation is held to restore peripheral sensitivity in spite of continued auditory action (**94**; **172**, p. 118; **295**; **296**). Instances of heightened peripheral sensitivity have also been claimed (**304**, **319**, **345**), but later investigators have attributed these departures from the general rule to deviations from the subject's normal physiological state (**137**; **188**; **274**, pp. 186–187). However, exposure to ultrasonic frequencies (e.g., 32,800 cycles/sec.) has been reported to increase peripheral sensitivity (**188**).

As in the case of macular stimulation, the same two-phase sequence of initial depression and subsequent elevation of peripheral sensitivity above normal—the latter phase a rather prolonged one—is to be discerned with auditory stimulation. This is true also when ultrasonic frequencies are employed (**188**; **274**, p. 190).

Olfactory stimuli of various kinds have been observed to influence the level of peripheral sensitivity of the visual organ. The odor of bergamot oil and of pyridine in toluol, for example, are held to heighten peripheral sensitivity, although, where there were strong negative reactions to the latter, instances of lowered sensitivity are reported. Spirits of hartshorn are also said to increase peripheral sensitivity (**48**, pp. 281–286; **161**; **183**; **260**; **274**, p. 188; **345**).

Similarly, gustatory stimuli of various kinds have been observed to influence the level of peripheral sensitivity of the visual organ. Weak sweet, salt, and acid solutions bring about an increase in peripheral sensitivity, whereas quinine brings on a decrease. The latter effect is, as above, attributed to the negative reaction of the subject to the bitterness of quinine (**161**, **186**, **345**).

Stimulation of the cold receptors is reported to produce a marked increase in peripheral sensitivity of the visual organ. For example, one minute after facial application of cool water, peripheral thresholds drop markedly. On the other hand, stimulation of the warmth receptors has a reverse effect, raising peripheral thresholds (**66**; **76**; **172**, p. 91; **183**).

The effect of vestibular, intero-, and proprioceptive stimulation on peripheral sensitivity has been the subject of a number of studies. Interoceptive stimulation, arising from uteral expansion and distension of the bladder, is held to affect peripheral sensitivity adversely (**172**, pp. 109–

110; **185**; **190**; **191**). Proprioceptive stimulation, evoked by muscular activity, is reported to induce varying effects: after light muscular activity, such as finger tapping at maximal rate or two minutes of gymnastic drill, an increase in peripheral sensitivity is noted; after heavy, fatiguing activity, however, a decrease ensues (**34–40**; **89**; **91**; **159**; **172**, pp. 91, 110–113; **185**; **433**). Postural stimuli exert varying effects on peripheral sensitivity of the visual organ: maximal sensitivity is elicited when the subject is comfortably seated; for the standing position it is considerably depressed (**172**, p. 118; **267**). Vestibular stimulation—from rotation of a seated subject five or ten times—produces a significant decrease in peripheral sensitivity, with return to normality requiring periods ranging from 5 to 30 minutes (**14**, **96**).

Dark Adaptation

Not only is peripheral sensitivity affected by various types of accessory stimulation, but so is the rate of dark adaptation. An accelerating influence on the process of dark adaptation has been reported from accessory stimulation of the cold, taste, and proprioceptive receptors; a similar effect upon accessory illumination of the eye with red light has also been reported (**67**; **157**; **172**, pp. 53–61; **288**; **313**; **316**; **319**; **401**; **414**).

Absolute Sensitivity of Central Vision

Considerable inquiry has been made into the effect of accessory stimulation on central visual sensitivity. From the research that has been done on this problem, it has been ascertained that auditory stimulation of moderate intensity heightens central sensitivity to white light for the dark-adapted eye, but that if monochromatic light is employed, the effect varies with the wave length utilized. Thus, central sensitivity of the dark-adapted eye to blue-green colors is raised through auditory stimulation, whereas, to orange-red, it is decreased. Central sensitivity to extreme spectral red and violet, as well as to the yellow portion of the spectrum near 570 mμ, remains unchanged however (**172**, p. 118; **225**; **227–234**; **244**; **250**; **404**).[8]

Kravkov, one of the foremost Soviet researchers in the field of sensory interaction, was able not only to demonstrate these effects over a considerable range of loudness, but also to show that the degree of effect varied directly with intensity of the accessory stimulus (**244**; **250**; **274**, pp. 270–273). He ascertained, further, that the observed changes in central sensitivity of the visual organ were a function of not only the intensity of the auditory stimulus but also of its duration. Thus, in an experiment to gauge the effect of sound on color sensitivity to green light, Kravkov found no effect from an auditory presentation of one-minute duration; however, he did note a pronounced effect on color sensitivity to green light following three minutes of auditory action.

The opposed character of color sensitivity to green and to red lights which is revealed when employing sound as accessory stimulus is disclosed also in experiments not involving auditory stimulation (**263**, pp. 35–39). For example, when the head is tilted back (postural accessory stimulation), it has been found that color sensitivity to green light (520

[8] Reverse accessory effects have also been noted on the central sensitivity of one eye when the other has undergone adaptation to red or green light. Adaptation of one eye to green light is reported to depress central sensitivity in the other upon accessory auditory action; adaptation to red light to elevate it (**143**).

mμ) is always markedly decreased, proceeding after 90 minutes to 25 per cent of its original value, whereas color sensitivity to orange-red light (610 mμ) exhibits an occasional tendency toward increase (**267**).

The direction of these effects is the reverse of that elicited with sound as the accessory stimulus. However, chilling, thermal, and gustatory accessory stimuli have been reported to induce directional effects similar to those elicited by auditory accessory stimuli. Here again, effects in the yellow region and the spectral extremes are not to be observed (**74**; **76**; **245**; **251**; **256**, pp. 48–49; **424**).

The opposed character of color sensitivity to green and red lights is demonstrated more fundamentally by experiments of a quite different type. Kravkov and Galochkina, for example, were able to demonstrate the opposing modifications of sensitivity to green and red colors by passing a weak electric current through the eye with strengths of current ranging from 0.02 Ma. to 0.5 Ma. and times of action up to 15 minutes. Where anodal eye contact was employed, color sensitivity of the dark-adapted eye was increased for green-blue illumination, but decreased for orange-red. On employing cathodal eye contact, the effects were reversed. No change was noted as regards the extreme ends and yellow region of the spectrum (**274**, pp. 281–282; **280**; **283**). The effect of auditory stimulation on central sensitivity of the eye was thereby shown to be similar to that resulting from anelectrotonic influence—a fact to which considerable significance is attached.

Differential Sensitivity to Brightness

Several experiments have been performed which produced evidence of an effect of accessory stimulation on the differential sensitivity of the eye to brightness (**70**, pp. 216–220; **98**; **99**; **274**, pp. 216–220; **285**; **292**; **293**). Illumination of one eye, it seems, makes for a simultaneous decrease in differential sensitivity of the other eye. Furthermore, the decrease is greater, the brighter the white-lighted field on which the latter is focused. The same effect can be obtained by keeping the brightness of the test field constant and increasing the brightness of accessory illumination (**85**, **86**, **88**, **216**, **218**).

Similar effects are elicited with sound as accessory stimulus. Thus, the brighter the viewed field, the greater the decrease in differential sensitivity under the effect of simultaneous auditory stimulation (**274**, p. 219); and very loud noise, such as that produced by an airplane motor, impairs markedly general brightness discrimination (**32**).

Accessory stimulation of the peripheral and central areas of the retina has also been shown to affect subsequent differential sensitivities. Thus, weak illumination separately of the peripherally distributed rods and cones acts to increase respectively the differential sensitivities of the centrally distributed cones and rods, while adversely affecting respectively those of their centrally distributed counterparts (**274**, pp. 220–221; **291–294**).

Critical Flicker Frequency

Sensory interactional effects are evidenced also in the results of studies on critical flicker frequency (c.f.f.) under the influence of accessory stimulation (**21**; **76**; **219**; **220**; **246**; **274**, pp. 357–370).[9] Kravkov, for example, noted that illumination of one eye with moderately bright light raises

[9] As was to be expected, accessorily induced changes in photic sensitivities parallel those noted in c.f.f., but with opposite sign (**274**, p. 365).

c.f.f. in the other, but wards off the reduction of c.f.f. that is usually observed in central vision of the other eye in the course of dark adaptation (**274**, p. 363).

It was demonstrated, in addition, that the effects of auditory stimulation on c.f.f. depend on the monochromatic nature of the light used. Thus, during the time of auditory action c.f.f. for green light (520 mμ) is reduced, whereas for orange-red (630 mμ), c.f.f. is raised. Here central retinal illumination was of such brightness as to produce critical flicker frequencies of 12–20 flashes per second. Accessory stimulation was pitched at 800 cycles at 85 db. On employment of white light as the primary stimulus, accessory auditory action was likewise found to affect c.f.f.: for central vision it is heightened, for peripheral vision lowered (**220**; **246**; **256**, pp. 49–50; **274**, pp. 364–365).

Experimenters, employing the odors of bergamot oil and geraniol, ascertained the effect of olfactory stimulation on c.f.f. (**251**; **274**, pp. 273–275). With brightness of flicker acting upon the central retina so as to produce a c.f.f. of 12–18 flashes per second for an eye at a given level of dark adaptation, the reduction of c.f.f. for green-blue illumination and its increase for orange-red under olfactory stimulation were established. No effects were noted for yellow (around 570 mμ) or for extreme spectral red (690 mμ) and violet (425 mμ). Furthermore, upon cessation of accessory action, it was found that c.f.f. returned not only to previous levels, but having done so, frequently proceeded to exhibit relative frequencies the reverse of those previously obtained under olfactory influence. A similar picture of change in c.f.f. has also been disclosed upon accessory application of gustatory and thermal stimuli (**76**).

Reverse effects have been obtained by varying the intensity of light employed. Thus, if brighter light should be employed to bring the corresponding c.f.f. to 26–30 flashes per second, then under the action of the same accessory stimulation c.f.f. may change in the opposite direction to that obtaining for brightness marked by a c.f.f. of 12–18 flashes per second. In other words, if c.f.f. for weak light is reduced by accessory stimulation, then much brighter light may under the same conditions increase it (**274**, pp. 367–368).

Visual Acuity and Irradiation

A number of studies have been devoted to research on the effect of accessory stimulation on visual acuity (**119**; **209**; **210**; **214**; **215**; **233**, p. 472; **256**, p. 50; **265**, pp. 26–29; **276–278**). These show, for example, that where closely set, black forms are to be distinguished from each other against a white background, visual acuity is sharpened under accessory stimulation; but where the shades are interchanged, it is diminished. Of interest also is the fact that, upon monaural stimulation, especially marked effects are to be noted on the visual acuity of the contralateral eye (**274**, p. 187).

In irradiational terms one can speak of the increase of irradiational effect under accessory stimulation. Visual acuity in the above case may then be thought of as enhanced because, through increased irradiational effect under accessory stimulation, the white interspaces between the black forms crowded on a white background are rendered larger and the black forms are consequently better brought out. Where the shades are interchanged, the reduction of the black interspaces under accessory stimulation would explain the resulting diminution of visual acuity (**214**, **215**, **276–278**).

Whether the accessory stimulation be through light, sound, or odor, the increase of irradiational effect for white is demonstrable in every case. For example, increase of irradiational effect is induced by the odor of bergamot oil (**214, 251**), and under accessory auditory stimulation irradiational effect for orange-red increases (**267**, p. 34)—a finding which might not have been anticipated in view of the reputed decline of color sensitivity to orange-red under auditory stimulation. This fact is considered to be of importance in surmising a probable mechanism behind the action of accessory stimuli on visual afferentiation, and we will return to it later.

Electrical Sensitivity of the Eye

Electrical sensitivity of the eye[10] has been shown to be modifiable by the action of accessory stimuli (**30**, pp. 162–164; **70**, p. 188; **94**; **139**; **410**; **412**; **415**; **462**; **463**; **479**). For example, illumination of one eye always raises the electrical sensitivity of the other. Auditory stimulation likewise increases electrical sensitivity, provided the sounds employed are not extremely loud, in which case electrical sensitivity is decreased (**20, 30, 130, 132**). Gustatory stimulation produces varying effects on the electrical sensitivity of the eye. Where sugar furnishes the accessory stimulus, electrical sensitivity is increased; where the stimulus is salt or citric acid, it is reduced. Under the olfactory action of bergamot oil increase in threshold of electrical sensitivity is reported to result (**71, 72**).

Color Zones

The boundaries of color zones have been reported to shift under accessory stimulation. Thus, with lateral stimulation of the retina with bright light expansion of the red and yellow color zones and contraction of the green and blue zones result (**105**). With sound as accessory stimulus the zonal boundaries for green and blue are enlarged, those for orange-red drawn in, while those for extreme red remain as they are (**144–146**)—effects which are reminiscent of the changes, previously described, in color sensitivity brought on through accessory auditory stimulation.

Olfactory stimuli also have effects on the zonal boundaries (**48**, p. 281). If the odors of rosemary and indol are employed, expansion of the green region results with the former and its contraction with the latter. As regards the red zone, rosemary is found to contract, but indol to expand it—effects the reverse of those above. However, alteration of red zonal boundaries is not always observed (**274**, p. 275; **398**; **399**, pp. 389–396).

Contrast

Changes in visual contrast effects under the influence of auditory stimulation have also been the subject of investigation (**446**). The effects vary with the degree of contrast obtaining prior to accessory action. Thus, if a gray circular area on a white background is presented with a tone of 800 cycles at 75 db as accessory stimulus, contrast is heightened if the initial difference of brightness between the gray circle and white background is in itself great. On the other hand, contrast is diminished if the difference is initially small.

Afterimages

Accessory auditory stimulation has been shown to affect the course of extinction of visual afterimages (**363, 364, 469–472**). In a number of

[10] Electrical sensitivity of the eye is inversely indexed by the minimum strength of the applied current necessary to induce visual sensation (phosphene) on closing and breaking the circuit. Thus, the weaker the current inducing phosphene, the higher the electrical sensitivity of the eye.

people, for example, a strong auditory stimulus heightens the brightness of the afterimage, though hastening the termination of its course. Here the accessory stimulus precedes the inception of retinal illumination and is continued until extinction of the resulting afterimage. Sounds that are powerful enough to evoke fairly intense auditory afterimages tend especially to heighten the brightness of the visual afterimage. However, should an extremely loud tone be presented after the cessation of retinal illumination, the afterimage undergoes temporarily complete disappearance.

Auditory Sensitivity

While most research has employed light as the primary stimulus, a number of studies have been undertaken with sound performing the role. Here again effects of accessory stimulation have been demonstrated. Illumination of the eyes with white light, for example, is claimed to increase auditory sensitivity; absence of visual stimulation, on the contrary, decreases it. However, opposed effects are obtained by using, as accessory stimuli, different monochromatic lights. Thus, illumination of a white room with green light increases auditory sensitivity; but with red light it is decreased (**182**; **202**; **312**, p. 155; **351**; **429**).

Auditory thresholds are lowered also when the odors derived from geraniol and benzol supply the accessory stimulation (**48**, p. 281; **134**; **267**; **379**; **380**). Postural stimuli likewise are reported to affect auditory sensitivity. Thus, on tilting the head backward for any length of time auditory sensitivity is found to suffer. It is also reported that higher auditory sensitivity is obtained in the seated position than in either the standing or horizontal position (**172**, p. 118).

Gustatory stimulation has been reported to improve auditory reception of low tones, but to worsen somewhat that of high tones. Established also have been the facilitating effects on hearing of accessory stimulation of the cold and proprioceptive receptors (**172**, p. 82; **366**).

Miscellany

Comparatively little work has been done where the primary stimulus has not been visual or auditory (**327**, p. 763). But what work has been done shows the presence of accessory effects. Thus, it is averred, illumination of the eyes with white light heightens gustatory and olfactory sensitivity (**48**, pp. 253–254), whereas auditory stimulation reduces the electrical sensitivity of the gustatory apparatus (**194**; **267**, p. 50). Electrical sensitivity of the gustatory apparatus, as well as its sensitivity to salt solution, is reported also to decrease in the course of dark adaptation of the eye. Mental activity, however, excludes the latter effect (**48**, pp. 140–141; **71**; **72**; **457**).

Research on the accessory effect of the odors of geraniol and thymol on the vestibular apparatus discloses reverse effects. The odor of geraniol heightens vestibular excitability, prolongs rotary postnystagmus, lengthens the illusion of counterrotation, and shortens chronaxie, while that of thymol has the opposite effect (**48**, pp. 281–282; **150**; **151**).

Some Conditions Affecting Sensory Interaction

Strength of Accessory Stimulus

Increase in strength of the accessory stimulus leads so frequently to effects that are the reverse of those induced by weaker intensities that

one can almost speak of a "rule of inversion" (**67**; **111**; **161**; **172**, pp. 80, 88; **178**; **184**; **226**; **263**, pp. 39–41; **274**, pp. 180–181, 190; **344**; **448–451**; **474**). For example, sounds of weak intensity heighten the electrical sensitivity of the eye, while those of increasing intensity are accompanied by a gradual decline. However, Kravkov doubts the generality of any "rule of inversion" and points to one case where the rule itself is controverted. Varying the intensities of sounds employed as accessory stimulation, he found that an increase from 25 db to 95 db was paralleled by a steady increase of sensitivity to green light (530 mμ) and a steady decrease to orange light (590 mμ)—findings which deny a general "rule of inversion" (**267**, p. 93; **274**, p. 180).

Excitatory State of Primary Sense Organ

The effect of accessory stimulation depends not only on the intensity of the accessory stimulus, but also on the initial degree of excitation of the sense organ undergoing primary stimulation. Thus, in order to increase the loudness of a tone on accessory visual stimulation, the initial loudness must be sufficiently great. Otherwise, either no effect will be demonstrable or a loss in loudness will ensue. The example cited under *Contrast* in the previous section is another instance of the effect of initial degree of primary excitation.

Duration of Accessory Stimulation

Intensity of accessory stimulation is not the only factor of importance; duration is also important. Kravkov (**130–132**, **184**, **344**, **348**) has shown that, as a rule, accessory effect becomes increasingly prominent during the first moments of accessory action. But subsequently the effect very often drops away from the maximum attained (**274**, p. 186). For example, accessory auditory stimulation was applied for a half hour while the eye underwent simultaneously 31 to 61 minutes of dark adaptation. Critical flicker frequency was found during this period to rise sharply, but to diminish after a quarter hour from the maximum previously attained (**220**).

Termination of Accessory Stimulation

In describing the influence of accessory stimulation on a given sense organ, it is, of course, necessary to stipulate whether the state of the latter is tested before or after cessation of accessory stimulation, since, after withdrawal of the accessory stimulus, its effect does not just wear off with an eventual return to normalcy. There frequently arises instead a shift in a direction opposite to that of the initial effect. Lowered sensitivity may be followed by a supernormal phase, heightened sensitivity by a subnormal (**157**; **274**, p. 190).

For example, Kravkov observed that sound lowers c.f.f. for peripheral vision. After its termination, however, he noted that under the same experimental conditions c.f.f. is raised, sometimes markedly (**220**; **274**, p. 364). As a further illustration, another experiment may be cited (**405**). Here, on testing the peripheral sensitivity of the eye after cessation of auditory stimulation, it was observed that a supernormal phase emerged subsequent to the decrease generally registered during the action of the accessory stimulus.

Affectivity of Stimulus

It appears that stimulus affectivity is a factor to be reckoned with in sensory interaction. For example, it has been claimed that, where gusta-

tory and olfactory stimuli of unpleasant character are applied as accessory agents, peripheral sensitivity of the eye declines (**172**, p. 88; **345**). It is also reported that, after presentation of harmonious tones, color sensitivity of the dark-adapted eye to red and yellow is raised, to green-blue lowered, whereas disagreeable sounds reverse the effects (**259, 426, 428**).

Physiological State

The specific physiological state of the sense organs, as well as the more general physiological state of the individual, plays a role in sensory interaction (**274**, p. 169; **399**, pp. 412–421; **455**).[11] A number of experiments may be mentioned by way of example. In a previously cited experiment, it was shown that presence of a subject in a green-illuminated white room heightens auditory sensitivity, whereas presence in a red-illuminated white room brings about a decline. However, on changing the physiological state of the subject through moderate dosage of veronal, the experimental effects are precisely reversed. In the hyposthenic state induced by veronal, green illumination lowers, red illumination heightens auditory sensitivity (**189, 429**).

Similarly, it has been demonstrated that with intense auditory stimulation the electrical sensitivity of the light-adapted eye undergoes decrease, and that of the dark-adapted eye, increase (**20**; **139**; **274**, p. 188; **303**). Electrotonic stimulation of the eye with anodal contact thereon (anelectrotonus) induces similar effects; with cathodal contact (catelectrotonus), however, reverse effects show up (**274**, p. 190; **411**).

Instillation of adrenalin into the eye has likewise been shown to bring on similar modifications in the electrical sensitivity of the eye: for the dark-adapted eye, electrical sensitivity declines; for the light-adapted, it increases (**106, 303**). Similarly, various accessory effects on electrical and color sensitivity can be made to reverse sign depending on whether the tested eye is light- or dark-adapted or whether it is adapted to green or red light (**235, 240, 249, 268, 298, 415**).

Miscellany

Summation of accessory effects. If each of two sense modalities, on accessory stimulation, induces an increase in peripheral sensitivity of the eye, their joint action induces an effect beyond that induced singly. If, however, the individual effects are in opposite directions, their joint action induces a general effect on peripheral sensitivity equal approximately to their difference (**172**, pp. 80–81; **181**).

Repetition of accessory stimulation. Constant repetition over equal intervals of time of the same accessory stimulus has been found frequently to yield a diminishing effect which tends to zero (**172**, p. 81).

Cumulation of accessory effects. The effects of accessory stimulation linger in some instances so that on subsequent days the change in level of primary threshold is found to undergo successive increase (**172**, p. 81).

Diurnal variation. The respective sensitivities of the rod and cone apparatus exhibit an inverse reciprocity of accessory influence that is diurnally dependent. Thus, it has been shown that at midday, when central sensitivity is at its maximum, peripheral sensitivity is minimal. As a normal relation, degree of sensitivity of the one apparatus is inversely tied to

[11] In various hypodynamic states, for example, the more usual effects of accessory stimulation tend to sharper expression (**172**, pp. 97–117).

degree of sensitivity of the other (**431**, **436–438**).

Some Theory on Sensory Interaction

In the attempt to comprehend the nature of the modifications of sensory response induced through accessory stimulus action, several considerations have been put to theoretical work. The result to date appears more a collection of eclectic plausibilities than an integrated set of relevant explanations. The following discussion will report what these considerations are.

Contiguity

Over a considerable range of auditory and visual stimuli certain visual and auditory responses undergo intensification through respective accessory reinforcement via intermodal action. To explain a number of them, such as the increase in visual acuity of one eye upon monaural stimulation of the contralateral ear, recourse has been had to the neurological map. In the region of the *corpora quadrigemina*, for example, the visual and auditory nerve fibers are in close proximity and myelin-free. Since nerve action in one unmyelinated fiber is held likely to influence that of another contiguous to it, the feasibility of excitatory irradiation through contiguity of the conducting fibers is considered to be demonstrated. The fact and feasibility of excitatory irradiation in the region of the *corpora quadrigemina* and also of the *corpora geniculata* seem to be justified, in the main, by reference to foreign work (**6**; **121**; **152**; **156**; **265**, p. 29; **274**, pp. 187–188; **304**; **319**).

Similar considerations are thought to make olfactory influence on vision understandable, for in the region of the *stratum zonale* and *nucleus anterior thalami* the visual and olfactory nerve fibers lie in close proximity. Thus, the olfactory afferent system may likewise have its effect on visual phenomena through excitatory irradiation.

The fact that there is anatomical proximity along the auditory and visual nerve pathways is also thought to make explicable many cases of synesthesia. On this basis the latter can be viewed as instances of sensory interaction (**267**, pp. 59–63).

Neural Excitation

The question arises as to whether it is better to view many instances of accessory effect as mediable through change in neural excitation rather than through change in neural excitability, that is to say, through threshold modification, although either alternative could conceivably account for a number of the intensifying effects produced by accessory stimulation. Kravkov and others, as a matter of fact, designed a number of experiments to throw light on just this point (**265**, pp. 26–29; **274**, pp. 222–230).

It had been established earlier that the sensitivity of central vision to orange-red light declines with simultaneous auditory stimulation; that is to say, a rise of threshold or decrease of excitability takes place. Kravkov reasoned that, if on accessory auditory stimulation positive irradiation could, nevertheless, be demonstrated to intensify for an object of orange-red hue (if its apparent size was increased), then change not in neural excitability, but in neural excitation must be decisive for achieving the observed effects. If positive irradiation decreased, then change in neural excitability could be taken to operate as the major factor.

Experiment realized the former contingency—the effect of positive

irradiation was increased, a result leading to the conclusion that accessory auditory stimulation may add to visual excitation of certain kinds (**257**). Thus, on this basis, the data, relevant to both positive irradiational effects and visual acuity on accessory auditory stimulation, are held explicable.

Further support for the emergence of an accessory effect through change in neural excitation is derived from the fact that with accessory auditory stimulation increase of differential threshold is not accompanied by a seemingly necessary decrease of irradiational effect. It is held that this failure of prediction is also accounted for on the assumption of an accessorily induced upward modification of neural excitation which renders $\Delta C/C$ greater in magnitude than $\Delta I/I$, of which it is the "central physiological correlate, . . . C and ΔC being measures of excitation in the nervous centers" (**274**, pp. 222–225).

Leveling and Accentuation

Under the "rule of leveling and accentuation" a refinement, implicit in the foregoing, was explicitly introduced to allow a wider encompassing of disparate data (**257**; **263**, pp. 31–35; **265**, pp. 26–29; **274**, pp. 225–230). This refinement consists in taking note of the initial contrast between the viewed object and its background. The following discussion points to this necessity.

It was shown that, with simultaneous auditory stimulation, visual irradiation of a white object against a black background intensifies, while that of a gray object against the same background diminishes (**215**). Here is an example of the kind of oppositely directed effects for which a unified explanation is needed. The rule of leveling and accentuation that has been proposed aims to provide this: where initial contrast is large, accentuation of contrast occurs; where it is small, leveling or reduction of contrast results. The reasoning behind the rule is fairly direct and may be indicated by way of example.

Supplementary excitation of the visual apparatus on accessory auditory stimulation is not distributed uniformly over the visual field but is unevenly apportioned. Those regions of the visual apparatus which already are to a considerable degree excited, "attract to themselves" the major portion of the additional excitation; the less strongly excited regions share more modestly. In other words, the magnitude of supplemental excitation, accessorily induced, is a function of the excitation directly occasioned by the primary stimulus. Thus, on accessory visual stimulation loud tones should sound louder; weak tones should be little, if at all, affected. In this way the rule of accentuation is held explicable.

But why a rule of accentuation *and* leveling? Again an example will elucidate. It is known that differential thresholds for brightness discrimination increase with brightness. If there is little initial brightness contrast between object and background, accessory increase of excitation is about equally shared, so that, where the ratio of differential brightness may have been I_2/I_1, it is *centrally* now $I_2+\Delta E/I_1+\Delta E$; ΔE is taken as the "brightness equivalent of the supplementary excitation" and $I_2>I_1$. This leaves the resulting difference in excitation about the same. The magnitude of the latter ratio, now less than the former, indicates, however, an impairment in sensing the brightness difference that obtains. Thus, brightness difference between object and background, initially contrasting little, comes to be even harder to ob-

serve with the result that a leveling tendency in brightness difference establishes itself (**274**, p. 225).

On the the other hand, should the initial contrast be very large, accessory increase of excitation accrues, in the main, to the brighter source as previously explained, so that the effect of contrast comes in this instance to be seen as exaggerated (**353**, **446**).

Similar reasoning would account for the oppositely directed shifts in critical flicker frequency that are to be observed when accessory auditory stimulation is employed to reinforce flashes of low and high brightness respectively. Critical flicker frequency is increased when the brightness of flash is initially high; it is decreased when initially low. Here the flash is thought of as an object presented against a less bright background represented by the fading afterimage between flash phases (**274**, pp. 366–370).

Intracentral Mediation

An accumulation of experimental data (**274**), as well as the anatomical richness of intracentral connections, made natural the consideration of intracentral mediation as a factor in sensory interaction. Certain specific research seemed also to suggest the intracentral mediational character of accessory action. For example, in the course of registering the bioelectrical currents in the auditory region of the cerebral cortex of rabbits, it was discovered that these currents are modified in an interesting fashion, should the eye of the animal be exposed to regular flicker: the rhythm of electrical potential in the auditory region comes, after an interval, to correspond to that of the flicker (**332**). In addition, the facts of protopathic and epicritic sensitivities were taken also to demonstrate the presence of central inhibitory action of some afferential systems on others. Thus, the tactile and proprioceptive systems, taken as phylogenetically of more recent origin, are seen as antagonistic in action to the systems giving rise to those sensations of gross temperature and diffuse pain which appear on disturbance of the inhibitory action of the former (**265**, p. 29; **327**, p. 611; **371**; **372**, pp. 39–53; **375**).

The widespread influence of the Pavlovian propositions of cortical action (since 1950 official dogma) has also had its effect in increasing the plausibility of intracentral mediation of accessory action (**274**, pp. 178–180; **338–340**; **374**, pp. 136–296). Thus, on the assumption of such mediation two possibilities present themselves: either excitation of a cortical center leads to excitation of another, modally distinct, or it leads to depression of the latter. Accessory effects may, therefore, be reinforcing or antagonistic.

It has previously been pointed out that central illumination raises peripheral threshold, while peripheral stimulation decreases central sensitivity. The relationship is mutually antagonistic, provided central vision is not that of the achromat, in whom an antagonistic influence is not exhibited due to cone deficiencies. Since central illumination of one eye, for example, affects adversely peripheral sensitivity in the other, it has been concluded that such effects, as indicated above, must, therefore, be intracentrally mediated (**391**).

Intracentral mediation of accessory action functions also to account for many of the reverse effects elicited upon increase in strength of the accessory stimulus ("rule of inversion"). The assumption is made that an initially weak accessory stimulus sets up two central proc-

esses: one positively inductive; the other negatively inductive. Upon increase in strength of the accessory stimulus, increases in the magnitude of the two oppositely signed inductive effects are given by two differently accelerated growth curves. Reversal of modal accessory effect is associated, therefore, with the change of sign resulting from algebraic summation of the two curves, once a certain magnitude of the accessory stimulus has been exceeded (**274**, p. 181).

Ionic Balance

It has been pointed out that under anelectrotonic influence central sensitivity of the dark-adapted eye to green-blue is increased, to orange-red decreased, whereas under catelectrotonic influence the reverse is observed. These changes parallel remarkably those resulting from the application of appropriate accessory stimuli and, hence, have been the subject of special study.

Attempts to explain the results of electrotonic influence have generally been based on considerations of ionic concentration and balance (**274**, pp. 281–286; **280**, pp. 138–143; **415**). In view of the known role of ionic calcium and potassium in the excitability of tissues, investigation turned first to them and later to ionic sodium and magnesium.

Since in a solution potassium ions are univalent and those of calcium bivalent, the former migrate to the cathode more rapidly than the latter upon application of direct current to a circuit. The value of the ionic index [K]/[Ca], which measures the relative concentration of ionic potassium and calcium, increases, therefore, at the cathode; it decreases, of course, at the anode due to increasing concentration of ionic calcium there.

If ionic factors are involved in the reported modifications of central sensitivity, it was reasoned that changes in the relative concentration of ions should be accompanied by changes in the various central sensitivities. Through iontophoresis[12] it was possible to introduce ionic calcium and potassium into the eyeball in order to test the surmise. Predictions were fulfilled (**274**, p. 284; **283**): ionic calcium was found to increase sensitivity to green but to decrease it to orange-red, whereas ionic potassium was found to increase sensitivity to orange-red but to decrease it to green. Termination of the iontophoretic process reversed the respective effects (**262**, **281**, **282**, **284**).[13]

Further experimentation showed that ionic sodium was similar in action to ionic potassium, while ionic magnesium was similar in action to ionic calcium (**260**, **376**, **377**). Hence, decrease in value of the ionic index [K]+[Na]/[Ca]+[Mg] is associated with increase in sensitivity of the green- and blue-sensing apparatus of the eye, while increase in value of the ionic index is tied to increase in sensitivity of the red-sensing apparatus.

Since ionic sodium and potassium are known to favor the concentration of acetylcholine in tissues, whereas ionic calcium and magnesium are known to lead to its reduction, it is considered very likely that various concentrations of acetylcholine exhibit respective optimal effects for the various color-sensing apparatuses of the eye (**274**, p. 284). In the last

[12] The introduction of ions into the body by means of electric current.

[13] Through iontophoresis it was found possible to neutralize certain accessory effects on central sensitivity. Thus, iontophoresis of sodium, which reduces central sensitivity to green (520 mμ), neutralizes the accessory effect of auditory stimulation which, under ordinary conditions, heightens this sensitivity (**275**, p. 146).

analysis, then, central sensitivities are seen as dependent on ionically influenced acetylcholine concentration (**275**, pp. 143–145).[14]

The Autonomic Nervous System

A variety of accessory stimuli such as tones, noise, the taste of sweetness, the odor of bergamot oil, geraniol, and camphor has been reported to produce similar differential effects on retinal sensitivity to green-blue and orange-red lights: heightening sensitivity to the former, decreasing sensitivity to the latter (**228**, **234**, **251**). This circumstance led researchers to look for a common factor behind this discerned uniformity of differential action. They noted that application of the above stimuli was accompanied by a quickening of the pulse, and it was conjectured that the common factor might well be the autonomic nervous system, in view of the fact also that the odor of thymol, which reverses the differential effects cited above, was found to reduce pulse rate (**8**; **9**; **160**; **172**, pp. 81, 88; **242**; **252**; **253**; **256**, p. 49; **265**, p. 31; **399**, pp. 412–419).

This conjecture among others led to experimentation which disclosed that the color sensitivity of the eye was changed in the reciprocal pattern mentioned above on direct instillation of adrenalin into the eye. Further research along these lines demonstrated that sympathomimetic substances such as cordiamin, phenamine, sympathol, and ephedrine heighten sensitivity to green light for the dark-adapted eye, but lower it to red light. On the other hand, parasympathomimetic substances such as carbocholin, veronal, the berries of *Schizandra Chinensis*, and pilocarpine usually bring on reverse effects: sensitivity to red light is heightened for the dark-adapted eye, while that to green light is lowered. Ephedrine is reported to heighten also gustatory, olfactory, and auditory sensitivities (**48**, pp. 255–258; **112**; **114**; **115**; **147**; **148**; **153**; **252**; **253**; **274**, pp. 276–278; **279**; **303**; **399**, pp. 401–407).

Confirmation of the autonomic mediation of a number of accessory effects has been sought also in data from experiments involving electrical application to the eyeball. For example, it will be remembered that if direct current of weak intensity is passed through the dark-adapted eye with anodal contact, sensitivity to green-blue light is heightened with corresponding diminution to orange-red light—the characteristic antithesis. The effects on sensitivities are reversed with cathodal contact (**268**; **274**, p. 190; **280**; **283**; **411**).

Now, it was pointed out that the relative concentration of calcium ions is increased in the vicinity of the anode during electrical action. Since increase in relative concentration of calcium ions, in a very great number of instances, is understood to induce effects similar to those of sympathetic excitation, the mechanism of the above accessory effects is held to be probably autonomic (**262**; **265**, p. 33; **274**, pp. 285–286; **282**; **284**).

Therefore, auditory stimulation, the odor of bergamot oil and geraniol, and the taste of sweetness are said to achieve their accessory effects through influence on the sympathetic

[14] Ionic calcium and magnesium are known to activize cholinesterase which through hydrolysis reduces acetylcholine content; ionic sodium and potassium, on the other hand, are known to suppress cholinesterase, thereby increasing acetylcholine content.

Interference with the acetylcholine chain of reactions, as through homatropinization of the eye, stabilizes central sensitivities in spite of iontophoresis. Hence, strictly speaking, not acetylcholine, but processes involving it in an essential way are to be associated with modifications of central sensitivities (**354**, **377**).

division of the autonomic nervous system with differential effects on the color-sensing apparatus of the eye; adverse effects are induced where parasympathetic excitation would instead ordinarily facilitate.

Corroboration of autonomic action has been sought in a variety of experiments. For example, backward tilting of the head for a period of time is claimed to bring about conditions favoring parasympathetic excitation. One should then expect a reduction of sensitivity to green light—an expectation which was confirmed (**265**, p. 32; **423**).

It should be noted that the differential action of the two divisions of the autonomic nervous system on the green- and red-sensing apparatus is also consonant with the observed reciprocal action of green and red light on a number of physiological functions. For example, the internal ocular pressure of one eye is increased or decreased according as the other eye is illuminated with red or green light respectively (**476, 477**). A similar reciprocity of action has also been observed with hyperventilation which decreases sensitivity to green light while increasing it to red (**296**).

Certain changes in peripheral vision are likewise held ascribable to modifications of the autonomic state (**8, 9**). Thus, vestibular stimulation, which is said to favor parasympathetic excitation, was noted to worsen peripheral sensitivity (**14**). Backward tilting of the head is also reported to decrease peripheral sensitivity, which fact accords with the increase of parasympathetic excitation held attributable to the tilted position of the head. Changes in peripheral sensitivity have also been noted with alteration of relative ionic concentrations, electrically induced—a fact which again is taken to relate modification in peripheral sensitivity to autonomic influence (**283**).

Further confirmation of the autonomic mediation of accessory effects was sought in experiments involving the cerebellum. For example, it was shown that subjection of the cerebellum to electromagnetic waves of ultra high frequency definitely altered the level of sensitivity for peripheral vision (**334**). Since, according to Orbeli, the cerebellum must be regarded as one of the main regulators of sympathetic nervous activity, the results of experiments such as the above were taken as tending to demonstrate the partial dependence of the visual apparatus on the autonomic nervous system (**372, 383**).[15]

While a wide variety of data is accounted for on the basis of autonomic mediation of accessory action, Kravkov points out that the initial drop in peripheral visual sensitivity on accessory auditory stimulation is an experimental datum which does not harmonize with all of the foregoing. Auditory stimuli appear to be sympathotropic as to effects: pulse rate is stepped up, the sensitivity of the green-sensing apparatus of the eye is heightened, the retinal blood vessels are constricted, etc. But peripheral sensitivity appears to be sympathotropically affected inasmuch as it increases under anelectrotonic conditions and decreases under vagotropic stimuli such as are provided by the backward tilting of the head. One should, therefore, expect a positive effect on peripheral sensi-

[15] Orbeli's theory of the "adaptive-trophic" regulatory action of the sympathetic nervous system on all tissues of the body, including the central nervous system and the striped musculature, has had a considerable influence on research in the field of sensory interaction (**55**; **69**; **95**; **123**; **160**; **162**, p. 77; **173**; **192**; **193**; **256**, p. 46; **334**; **336**; **337**; **368–374**; **456**; **468**).

tivity through accessory auditory stimulation. The reverse, however, takes place. To account for this, Kravkov hypothesizes the existence of intracentral connections whose influence in an *ad hoc* fashion dominates antagonistically over those influences proceeding through the medium of the autonomic system (**267**, p. 81).

The Green Receptors

The fact that the application of either sympatho- or vagotropic accessory stimuli leaves unaffected sensitivity both to extreme spectral red and violet and to colors in the region of 570 mμ has led to the assignment of an especial role to the green-sensing apparatus of the eye. It is hypothesized that only if the green receptors are stimulated is accessory heightening or lowering of color sensitivity evocable (**254**; **256**, p. 49; **266**; **274**, pp. 327–331; **352**).

Throughout the spectral range of excitability of the green receptors (from 430 mμ to 680 mμ approximately) excitation of the green-sensing apparatus appears everywhere accompanied by accessory effects except in the neighborhood of 570 mμ. Reference to the three excitation curves of the three basic color receptors (*b*, *g*, *r*) shows that the ordinates Y_g and Y_r are about equal in the neighborhood of 570 mμ, Y being taken as equal to degree of excitation. Since excitation of the green receptors is held to induce a positive accessory effect, the failure of accessory effect in the neighborhood of 570 mμ is ascribed to an initiated negative accessory effect of equal magnitude, induced by a simultaneous excitation of the red receptors.

Since magnitude of unsummated accessory effect is regarded as a direct function of degree of excitation of the respective color-sensing apparatus involved, where $Y_g > Y_r$, the algebraic sum of the associated positive and negative accessory effects yields a residual accessory effect which is positive; where $Y_g < Y_r$, the residual accessory effect is negative; where $Y_g = Y_r$, accessory effect is lacking ($430 < m\mu < 680$ approximately).

Excitation of the blue receptors is thought to enhance accessory effect on the condition that $m\mu > 430$ approximately. Thus, in the neighborhood of 520 mμ where $Y_b = Y_r$ approximately, the inhibiting action of the red receptors is canceled out by the sensibilizing action of the blue receptors. This produces, accordingly, a positive accessory effect derived solely from stimulation of the green receptors, one which, as it turns out, is maximal.

On making the inhibitory and sensibilizing action of the red and blue receptors respectively explicitly dependent upon excitation of the green-sensing apparatus, the failure to elicit accessory effects at the spectral extremes ($m\mu < 430$; $m\mu > 680$ approximately) is made understandable.

To test the hypothesis that accessory effects on color sensitivity in trichromats are not observed without excitation of the green-sensing apparatus, an experiment was performed to see whether through central mediation accessory effects at the spectral extremes could not be elicited in one eye upon illumination of the other with green light. The results were affirmative, thereby confirming the special role assigned to the green receptors (**253**, **254**, **352**).

Modification of Primary Conditions

Accessory stimulation is held to produce its effect in many instances in ways not involving considerations of excitation or excitability of the primary sensory system. These refer

to accessorily induced modifications of the physical conditions under which primary stimulation is applied. For example, the pupillary reflex may respond to an auditory stimulus. Here the quick contraction of the pupil with its slow subsequent relaxation would be expected to modify some aspects of the visual function. Since the amount of light entering the eye is affected by pupillary width, upon nonemployment of an artificial pupil apparent changes in visual threshold which follow upon auditory stimulation may be ascribable not to changes in excitation or excitability of the visual afferent system, but rather to changes, induced by accessory stimulation, in the physical conditions. Since the pupillary reflex is not without effect on accommodation, visual acuity also may undergo modification owing to the accessorily induced changes in the physical conditions accompanying primary stimulus action.

Similarly, pain, vestibular stimulation, etc. modify the pupillary reflex and thereby may give rise to simulated accessory effects. As another instance, tactile stimulation around the ear appears to affect auditory threshold; but again, the result of such stimulation might more aptly be ascribed to reflex changes in the small muscles controlling the tension of the bony system of the middle ear than to changes in the auditory afferent system (**267**, pp. 87–88; **274**, pp. 86–88).

Conditioning

The conditionability of sensory responses adds immensely to the range of sensory interaction and is considered a factor to be reckoned with though the accessory effect is not native but acquired. Work in the Soviet Union on conditioned "sensory reflexes" dates back to 1936 when Kravkov, Kekcheev, Bogoslovkiĭ, and Dolin independently demonstrated the conditionability of changes in visual responses. Thus, it was early shown possible to condition changes in electrical sensitivity of the eye, critical flicker frequency, peripheral sensitivity, etc. to such indifferent stimuli as ticking, a tone, and elapsed time of eye exposure to the dark (**16–18**; **22–25**; **50–52**; **57**; **79–84**; **124**; **169–171**; **183**; **202**; **233**, p. 474; **256**, p. 53; **259**; **263**, pp. 41–43; **265**, pp. 36–39; **267**, p. 35; **274**, p. 192; **309**; **339**; **381**; **396**; **417**; **418**).

To illustrate some of this work: Pshonik managed to induce changes in thermal sensation in response to accessory auditory stimulation by making of the latter a conditioned stimulus (**57**, **381**). Dobriakova conditioned increase of gustatory sensitivity to the ticking of a metronome after having associated the latter with light—a stimulus which ordinarily induces a positive accessory effect on gustatory sensitivity. In addition, it was found that symbols, denoting stimuli which heighten gustatory sensitivity, make manifest like effects (**71**, **72**, **171**). A general heightening of various visual and auditory sensitivities has been observed to ensue through mere presence of a subject in the experimental room. Frequent prior presence in the room while in a state of intense attention is here said to condition modification of visual and auditory sensitivities to the sight of the experimental room—the state of attention being thought of as in the role of "unconditioned stimulus" (**79**; **80**; **267**, pp. 85, 111; **272**; **274**, pp. 192–194).[18]

[18] "Concentrated attention" has been assigned an important role in the sensibilization of the receptors. "Attentive practice" and expectation also lead to sensibilization, the former being held capable of doing so on a permanent basis (**48**, p. 209; **149**; **172**, p. 92; **274**, pp. 230–231; **396**; **408**; **416**; **460**).

Concluding Remarks

Soviet research on sensory interaction has amassed over the past 24 years a fund of data that, in spite of a deserved skepticism, cannot but strike one for its persistent consistency and "logical," though novel, character. This research appears to demonstrate that all modalities undergo various modifications of sensory response on appropriate application of an accessory stimulus and that, where the primary stimulus is visual, the resulting modifications conform to striking patterns.

Basic to one of these patterns is the fundamental role assigned to the green-sensing apparatus whose excitation is a requisite for the mediability of accessory effects through simultaneous excitation of the non-green receptors. Of the latter there are those which upon excitation are reinforcing for accessory effects, while others are depressive. Thus, where sensitivity to blue-green, for example, is heightened, that to orange-red is lowered; and vice versa.

The antagonistic relationship between color receptors is traced to inversely related ionic concentrations induced through autonomic nervous action brought into accessory play. Thus, where ionic calcium predominates, sensitivity to green is heightened, to orange-red lowered; where ionic potassium predominates, reverse sensitivities obtain.

Other patterns of accessory effect and of reciprocal accessory action have been intensively investigated with characteristic results. However critical one may be of the Soviet studies, the cumulative results obtained with their apparent inner consistencies suggest, therefore, the worthwhileness of a re-examination of the whole problem of sensory interaction. Recent restrictions on types of psychological research in the U.S.S.R. in favor of "Pavlovian" methodologies and conceptions suggest that non-Soviet researchers are the ones to undertake the job (**60**; **201**; **272**; **338–341**; **434**, pp. 69–70; **473**; **498–503**; **505**).[17]

[17] Signs point to a possible relaxation of dogmatic prescription of allowable methodologies and conceptions. Both Ivanov-Smolenskiĭ and Bykov, the official "Lysenkos" of Soviet psychology, physiology, and related disciplines, who in their ruthless advocacy of a raw Pavlovism have exercised great power in these fields since 1950, have recently been censured in *Pravda* along with Lysenko for suppressing the views of those holding conceptions "deviant" from theirs (**440**).

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THE INTERRELATIONS OF PSYCHOLOGY AND BIOGRAPHY

JOHN A. GARRATY
Michigan State College

This paper is an attempt to summarize the interrelationships of two seemingly separate disciplines that deal with the study of man. Biography, in all its diverse forms, attempts to describe human lives, and this is also one of the purposes of the science of psychology. Inevitably, these two fields have drawn upon each other in a manner beneficial to both, though, as will be seen, each could make still better use of the other than it does at present.

From the earliest times, the best of the writers of biography have striven to describe not only the overt actions and recorded facts of their subjects' lives, but also their personalities. The more perceptive writers have also realized that there is a connection between a man and his deeds, that an understanding of his personality helps explain his accomplishments, and that his accomplishments throw light upon his personality. This insight is implicit, for example, in these famous lines from Plutarch's life of Alexander the Great:

> My design is not to write histories, but lives. And the most glorious exploits do not always furnish us with the clearest discoveries of virtue or vice in men; sometimes a matter of less moment, an expression or a jest, informs us better of their characters and inclinations, than the most famous sieges, the greatest armaments, or the bloodiest battles whatsoever (**69**, p. 801).

Thus, the best biographers, like the best novelists, have always been students of human psychology, and the development of psychological science has tended to confirm insights long ago achieved. The novelist E. M. Forster has written: "Psychology is not new, but it has newly risen to the surface. Shakespeare was subconsciously aware of the subconscious" (**35**, p. 9). It is hardly necessary to belabor this point, which has been made many times. C. K. Trueblood, for example, has elaborately described the sound psychological principles developed by the French critic-biographer Sainte-Beuve (**81**). Or, to cite an American illustration, the following statements taken from James Russell Lowell's sketch of Thoreau show obvious psychological understanding:

> He condemns a world, the hollowness of whose satisfactions he had never had the means of testing.
>
> Those who have most loudly advertised their passion for seclusion and their intimacy with nature . . . have been mostly sentimentalists, unreal men, misanthropes on the spindle side, solacing an uneasy suspicion of themselves by professing contempt for their kind.
>
> Thoreau had not a healthy mind . . . . His whole life was a search for the doctor (**57**, pp. 200, 205, 203).

But the tremendous development of psychology as an organized discipline since the late nineteenth century has naturally had an impact upon the writing of biography. By 1896 Havelock Ellis was calling biography a branch of "applied psychology" and bewailing the fact that most life writers knew nothing of the work of men like Wundt, G. Stanley Hall, Münsterberg, and Jastrow (**32**, pp. 96–97).[1] The publication of

[1] The average biographer, Ellis wrote, was producing "a figure that is smooth, decorous, conventional, *bien coiffé*, above all, closely cut off below the bust . . . " (**32**, p. 98). This was certainly an accurate picture of most (but not

Freud's *The Interpretation of Dreams* in 1900 and more especially of his *Leonardo da Vinci* a decade later opened up the possibility of psychoanalyzing historical figures, and of reinterpreting great masses of human history as well.

At first, the influence of the radical Freudian thinking spread slowly. With psychologists themselves hesitant or openly scoffing, biographers, if they were even aware of psychoanalysis, did not rush to adopt the technique. The earliest psychoanalytic biographies were written by European psychologists, followers of Freud, and were published chiefly in Freud's magazine *Imago*, and in other German professional journals (**30**). A notable exception to this was a study of Martin Luther by the American historian and Reformation scholar Preserved Smith, which appeared in 1913 and explained Luther's career in terms of the Oedipus complex (**78**). But Smith neither pursued the method further nor transferred his conclusions about Luther to his other writings about the Reformation. Few historians, therefore, were even aware of his effort, and thus not stimulated to copy it.

The development of the "new" history under the leadership of James Harvey Robinson after 1910, with its emphasis on broadening the interests and methods of history, did not neglect psychology. Robinson, addressing the American Historical Association in 1910, called for a general expansion of history into new fields. "History," he said, "must recognize that it is but one of several ways of studying mankind." Robinson called attention to Tarde's work in the field of imitation, and warned his colleagues that "the relations of our reason to the more primitive instincts which we inherit from our animal ancestors . . . will never be understood without social psychology" (**72**, pp. 74, 93). But while the "new" history was readily accepted by historians, historical writers and biographers did not hasten to apply psychological tools to their work.

A great change in this situation came about after World War I. The experience of doctors dealing with thousands of cases of war neuroses, which were obviously not the conventional "shell shock," did much to strengthen the Freudian theories. The appearance in English of Freud's *General Introduction to Psychoanalysis* in 1920, and the popularizations of psychoanalytic theory, spread the doctrine into nonprofessional areas (**64**, p. 329). Suddenly biographers began to see the possibilities—the insights into hidden motives, the escape from the limitations of "factual" biography, the sensationalism inherent in an approach that emphasized sex, with its healthy effect on sales. As early as 1919 Harry Elmer Barnes, a disciple of Robinson, had gone on record in support of the use of psychoanalysis in biography (**9**),[2] and by the next year the rush was on.

It would be of little value to attempt to list all the biographies published in the twenties that made conscious use of Freud and of his

of all) late Victorian biographies. Ellis' indictment is an interesting precursor of Lytton Strachey's famous assault on the type in the preface of *Eminent Victorians* written in 1918.

[2] Another early example of the influence of Freud on biography can be found in John Maynard Keynes's *Economic Consequences of the Peace* (1919). In discussing President Wilson's attitude toward the Versailles Treaty, Keynes wrote: "In the language of medical psychology, to suggest to the President that the Treaty was an abandonment of his professions was to touch on the raw a Freudian complex. It was a subject intolerable to discuss, and every subconscious instinct plotted to defeat its further exploration" (**52**, p. 49).

critics within the psychoanalytical movement like Jung and Adler, but a few of the most popular should be mentioned. Katharine Anthony's *Margaret Fuller* (7) was one of the earliest. R. V. Harlow's *Samuel Adams* (44), Harvey O'Higgin's *The American Mind in Action* (66), Joseph Wood Krutch's *Edgar Allen Poe* (53), Clement Wood's *Amy Lowell* (86), G. W. Johnson's *Randolph of Roanoke* (48), and L. P. Clark's *Abraham Lincoln* (21) are among the best known.

The appearance of such books as these touched off a debate among biographers, historians, literary critics, and others which rivaled the general contest between the advocates of psychoanalysis and the conservative forces of society. Naturally enough, the practitioners of the technique were loud in their own defense. Leon Pierce Clark, the biographer of Lincoln and himself a practicing psychiatrist, argued that the ordinary biographer "has not fully exhausted the possibilities of his subject, because of inadequate psychological training" (19) and cited "such competent [historical] authorities" as Robinson, Barnes, and Preserved Smith to prove that leading historians recognized the value of psychoanalysis (20).

The Freudian biographers tended to place great stress upon the close connection between a writer and his literary productions. They defended wholeheartedly their use of imaginative writings as source material for an understanding of the author's personality. Discussing Poe, Joseph Wood Krutch declared: "The forces which wrecked his life were those which wrote his books." The man and his works, Krutch argued, were "nearly identical" (53, pp. 18–19). Clement Wood, himself a poet, wrote in his *Amy Lowell:* "A poet's work must be studied in the light of the new psychology, with the certainty that . . . we will secure from the poetry a portrait of the author . . ." (86, p. 12).

Lewis Mumford, writing a "psychological" biography of Herman Melville (62), made heavy use of Melville's novel *Redburn* for material on the novelist's early life. Mumford has stated that a biographer who fails to take into account his subject's unconscious is guilty either of "ignorance or childish cowardice." While admitting that the method may involve surmises based on scanty evidence, he has argued that "it is better to make mistakes in interpreting the inner life than to make the infinitely greater mistake of ignoring its existence and its import" (63, p. 5).

Perhaps the supreme tribute to psychoanalytical biography was paid by the Marxian historian Matthew Josephson:

> The dialectics of subconscious motive underlying the conscious proved to be as fruitful in measuring the individual life within a milieu as the dialectics of interest versus idea in the interpretation of collective society (50, p. 100).

But against such opinions as these can be placed a mass of contradictory statements, for many biographers have attacked the method. First may be mentioned those who seemed to have been motivated chiefly by prudery. It is unnecessary to do more than quote a few lines to show the nature of their argument:

> The worst foe of biography that has yet appeared is the disciple of Freud, who crawls like a snail over all that is comely in life and art (82, p. 772).

> Apart from all their sex nonsense and their ridiculous reconstruction of their characters' psychology, [psychoanalytical biographers] . . . raise serious questions as to whether these authors quite realized who and what their heroes really were (1, pp. 26–27).

Actually, very few critics have resorted to such arguments as these. But many have based their opposition on a denial of Freudian principles, particularly his insistence upon the dominant importance of sex. George Bernard Shaw once wrote to Frank Harris, who was preparing a life of the playwright:

> O Biographer, get it clear in your mind that you can learn nothing about your sitter . . . from a mere record of his gallantries . . . . If I were to tell you every such adventure that I have enjoyed, you would be none the wiser as to my personal, nor even as to my sexual history (quoted in **15**, p. 755).

More specifically, Emil Ludwig wrote in *Dr. Freud*, which is a bitter attack upon psychoanalytical biography: "When Freud inspects a figure out of history he resembles a physician who instead of examining his patient's whole body looks at a single organ, usually the genitals, and upon it founds his diagnosis" (**58**, p. 177).

Less prejudiced critics have claimed that whatever its value as a therapeutic tool, the use of psychoanalysis serves no purpose in biography. Historian Jacques Barzun, for example, has argued that the typical Freudian biography merely substitutes a new jargon for old expressions, without really throwing light on the subject's personality (**11**, pp. 277–278). Similarly the biographer Phillip Guedalla once wrote of "the familiar cake iced thinly with a little mental slang imported from Vienna" (**41**, p. 115),[3] and Hesketh Pearson has made essentially the same point (**68**, pp. 313–315).

A powerful complaint raised by the foes of psychoanalysis in life histories has been the lack of evidence upon which to base the study of a personality no longer living. Dumas Malone, editor of the *Dictionary of American Biography*, has put it in its simplest terms. "It is a pity," he wrote in his essay "Biography and History," "that no more authentic information is available about . . . the crucial early years of nurture" (**59**, p. 143). André Maurois made the same point in his *Aspects of Biography*. "Who is keeping a record of Bertrand Russell's dreams, so that the Freudian biographers may interpret them at a later date?" he asked (**60**, p. 90). The anthropologist Alexander Goldenwiser, certainly sympathetic to psychoanalytic theory in itself, has written:

> It is, of course, tempting to apply the insights of psychoanalysis to a speculative interpretation of historical characters . . . . But the procedure is hazardous in the extreme and, more important still, it stands in contradiction to the avowed professions of the psychoanalysts themselves (**39**, p. 416).

Finally, the most perceptive critics have tended to concentrate their attack on the manner in which psychoanalytical biographers have used the Freudian constructs of sublimation and reaction formation. Harry Elmer Barnes, to cite a single objectionable instance, was able to explain Alexander Hamilton's "authoritarian" personality as resulting from the fact that he was never subject to the control of a stern father. But in the same article Barnes accounted for Jefferson's anti-authority complex by explaining that his father died when he was only fourteen—before his "father image" had had time to mature! "It is significant," Barnes commented learnedly, "that Jefferson's antipathy to his father was so infantile and deep-seated that it was scarcely ever raised to consciousness." Most of Jefferson's references to his father were "reverential and awe-inspired," which to Barnes was "obvious" proof

[3] Psychological biography, Guedalla wrote on another occasion, is "one half of something we all knew before, and . . . the other half of something which is not so" (**42**, p. 928).

that he hated him (**10**, pp. 28–37). It was this kind of analysis which led Howard Mumford Jones to complain: "So long as any symbol can be translated into any other symbol from the subject's past, so long the psychoanalyst may roam at will" (**49**, p. 114). Bernard De Voto, never a man to compromise a strongly held opinion, has even said: "Psycho-analysis has no value whatever as a method of arriving at facts in biography. No psycho-analytical biography yet written can be taken seriously—as fact" (**26**, p. 185). To which may be added the satire of Stephen Vincent Benét:

> And as for the poor devil's really loving his wife—well, I know that every authentic utterance of his on the subject says he did, and there are those dedications of his books, that sound fairly sincere. But honestly, he simply hated her all the time . . . . Don't you realize that his unconscious portrait of her as the degenerate scrubwoman with leprosy in chapter four of "Wedded and Wooed" represents his real feelings toward her? (**12**, pp. 169–170).

It is clear that some of the arguments against psychoanalytical biography have had great force. The initial enthusiasm of the twenties slackened off rapidly in the thirties, and while such studies still occasionally appear, they are relatively rare today. In a recent survey of British biography by V. S. Pritchett, only one book (a life of Emile Zola by Angus Wilson) was listed as utilizing "the Freudian approach," and American biography has followed the same trend (**70**). Examination of the *Psychological Abstracts* reveals a similar decline of psychoanalytical studies of historical personalities by psychologists. Indeed, Franklin Fearing pointed out as early as 1927 that few academic psychologists had ever actively espoused the method (**33**, p. 535).

The rejection of psychoanalytical techniques by most biographers has not meant a corresponding disavowal of all psychology. Some have argued that psychology is merely "human nature" and that no formal study of the subject is necessary,[4] but the number who have paid tribute to the importance of the subject is far larger. As early as 1912 the writer Allen Johnson, while protesting against merely speculative studies of personality, called for greater awareness of the complexities of human character. The biographer should pay more heed to the early life of his hero instead of "joyously transport[ing] the inchoate statesman into the national arena, where he will thenceforth shape the course of empire" (**47**, p. 394). Johnson also suggested that an intensive study of great men might throw light on the problem of leadership, an idea that modern psychology is certainly investigating (**47**, p. 403).

Later writers have touched upon related points. An English critic, after denouncing the way psychoanalysis has been "ridiculously abused" by biographers, has praised the "weapon of psychology" which provides the modern biographer with "a far better surgical dissecting instrument than his predecessor had" (**27**, p. 128). "The new psychology," this critic writes, "has infused the art with new blood" (**28**, p. 19). An American student of modern biography has put it this way: "I do believe that somewhere between the extremes of the conventional method and the purely psychological there lies a mean," while Claude Fuess, an experienced practitioner of the art,

[4] Hesketh Pearson, for example, has denied all formal knowledge of psychology. "Dr. Freud . . . is to me but a name." To persons who have claimed that his biographies show psychological insights, his answer is that this is "simply due to my own common sense and intuition" (**68**, pp. 313, 315). (See also **24**, p. 4.)

after examining the dangers of peering into the unconscious, concludes: "Yet with all these reservations, I am sure that the chief contribution made in recent years to the art of biography is the application of psychology to the interpretation of personality" (**36**, p. 351).

Most such statements, while well intentioned, are extremely vague. What these writers seem to be getting at in praising psychology as a tool of biography is difficult to tie down. But an effective summary has been offered by the historian Edward M. Hulme: Psychology has led to "the wholesome skepticism with which we have come to regard many of the alleged facts of history." It has made it possible to deal more satisfactorily with the conflicting statements of eye witnesses, with autobiographical writings and other personal documents, with human habits and motives—in short, "it helps to account for many facts, and also to trace the series of mental operations that come between the facts and the written record of their observation" (**45**, pp. 175–178).

Yet psychology can offer biography something more than this, though biographers have not availed themselves of it. In a few cases, where the evidence is plentiful and the conditions clear, psychoanalysis can certainly be used with justice. In the case of George III, who was clearly insane, and whose madness was thoroughly examined by contemporaries, the records are adequate, and a study such as Guttmacher's *America's Last King* is perfectly acceptable to the most conservative historians (**43**). On the other hand, it may be argued that where the ordinary evidence is extremely skimpy, as in the case of Leonardo, a psychoanalytic interpretation like Freud's may be quite useful. When conventional evidence is missing, the biographer must guess in any case, and an intelligent guess based on psychoanalysis, provided it is clearly labeled a guess (as Freud's was), can surely be defended.

Even in the vast majority of biographies, which fall between the two extremes, no one should object to the use of Freudian techniques if they are explicitly described as speculations, and if known facts are not twisted or ignored in order to bring the subject into a preconceived pattern. Much of the criticism that has been aimed at psychoanalytical biography has arisen from the dogmatic way in which the authors of such works have stated their cases. As Bernard De Voto has said: "*Must* is the mechanism of psycho-analytical biography. . . . Biography proper is not concerned with the *must* but only with the *did*" (**26**, p. 188). If psychoanalytical biographers would confess to the speculative nature of their conclusions, their critics would be less bitter.

Aside from psychoanalysis, biography could benefit from other psychological techniques for the study of personality, particularly from the scientific analysis of personal documents. This is a field that has been badly neglected. When the Social Science Research Council conducted its inquiry into the use of personal documents in psychology, history, anthropology, and sociology, its historian, Louis Gottschalk, begged the question and presented a paper which confined itself almost wholly to an analysis of general historical method. "For the historian the method of dealing with human documents is all of *historical method*," he wrote (**40**, p. 6), ignoring the host of ideas adaptable to historical study in G. W. Allport's "The Use of Personal Documents in Psychological Science" pub-

lished in 1942, three years before his own work appeared. Allport's cogent analysis should be inspected by every biographer, both for its summary of the work of students of personality and for its balancing of the pros and cons of the usefulness and reliability of personal materials (**5**).

Certainly there are many ways in which biographers could apply knowledge gathered by workers in psychology. Recent advances in the study of graphology, for example, ought to be considered carefully. While it would be foolish to interpret a historical figure only in terms of his handwriting, the rich ore that this mine of expressive movement can yield should be dug out, refined, and forged into a handy biographical tool, for, as Allport and Vernon have pointed out, handwriting is really "brain writing" (**6**, p. 187). Indeed, the entire field of projective and expressive techniques, ignored by biographers, should be searched for methods that might be applicable to the study of persons no longer living.

Unfortunately, the most valuable tools, like the Rorschach and the TAT, are of no use in historical investigations, but there are others that might well be employed. Certain forms of semantic analysis of both the written and spoken word could be applied to the expressions of figures from the past, for example, D. P. Boder's discovery that there are great variations in the use of adjectives and verbs in different *kinds* of writing, and also within the writings of individuals over the course of time. Boder's study of Emerson's journals and the letters of William James brought out many interesting relationships, such as the fact that the adjective-verb ratio in James's letters varied significantly according to the sex of his correspondent (**13**, pp. 340–341). The literature in this field has been summarized by F. H. Sanford (**74**, **75**), who concludes: "Characterizations of style appear also to be characterizations of persons" (**75**, p. 198), a point which biographers could ponder with profit.

There are other tools that might well assist the biographer in digging out the complexities of his subject's personality, for the most important developments in this area have gone beyond the counting of mere word forms. John Dollard and O. H. Mowrer have developed a means of measuring tension as expressed, often subtly, in personal documents (**29**). By counting the words, sentences, and "thought units" that express some form of discomfort (D), and those indicative of relief or reward (R), they have arrived at a Discomfort-Relief Quotient (DRQ), which can be determined by the equation $DRQ = D/D+R$. Thus, hidden emotional states may sometimes be uncovered. That this device might be valuable to biographers seems obvious. (See also **51**.)

Also of great potential usefulness to biographers is the study that Alfred L. Baldwin called "Personal Structure Analysis: A Statistical Method for Investigating the Single Personality" (**8**). Baldwin took a series of letters written by a single subject and classified them according to the goals, attitudes, and objects discussed. The resulting analysis was based on two principles: (*a*) The more frequently an item appears, the more important it is in the individual's personality structure, and (*b*) the mentioning together of items more often than would be likely according to chance indicates a relationship of these items in the personality of the subject (**8**, p. 168). Although a simple reading of the letters pointed up many such relationships, the mechanical counting and sorting of the ma-

terial highlighted a number of others which made the letter writer's personality (and behavior) more intelligible. If this technique was helpful to a trained psychologist, it would be even more useful for the average biographer.

Closely related to the Baldwin study is the method of "value-analysis" developed by Ralph K. White (**84, 85**). This technique is applicable to autobiographical writings; White developed it by studying Richard Wright's *Black Boy*. First he attempted to evaluate Wright's personality on the basis of an ordinary reading of the book. Then he applied value-analysis, going over it again classifying and counting each value judgment made by Wright. He uncovered more than half a dozen relationships, some of great importance, which he had missed in reading the book, but which, upon further thought, fitted well into the general picture he had formed (**84**).

The particular value for biographers of techniques like these is that they can be employed by nonspecialists. The proper recognition and classification of the material requires training it is true, but nothing like the work that must be put in by anyone seeking to use psychoanalysis. White claims that his method can be mastered in from ten to twenty hours of study and practice (**84**, p. 441), surely not an unreasonably long apprenticeship for a writer interested in getting inside his hero. Since these methods are plainly supplementary ones and futile to apply unless combined with subjective study, they ought not to offend the sensibilities of those who object to the employment of "mechanical" techniques in "artistic" problems. Some of these methods ought to be given at least a trial by biographers. Surely, as Saul K. Padover has written, to "illuminate the internal dynamics of a person" one requires "imagination, psychological insight, psycho-social techniques" as well as the conventional documents (**67**, p. 16).

At the same time, life writers must beware of overemphasizing the "scientific" approach to personality. The biographer can learn from the psychologist, but he cannot reduce the *presentation* of personality to a science.

In 1924, when the rage for the "new" Freudian psychology was at its height, the American Political Science Association appointed a committee which, among other things, investigated the possible applications of psychology to the science of government. The conclusion arrived at might well be accepted as the present judgment of the usefulness of psychology in biography. "Probably the general sentiment of the committee toward psychology," reported chairman Charles E. Merriam, "would be expressed by the phrase, *con amore ma non troppo*" (**61**, p. 469).

Just as the first biographers were intuitive psychologists, so the early philosophers, considering the basic questions of human psychology, were influenced by what they knew of men who had lived before them as well as by their own contemporary observations. Of course, much of this information came from biographies. But it was the publication of Darwin's *Origin of Species* in 1859 which first led to serious study of the lives of men of the past by psychologists. Environmentalists saw in the principle of natural selection proof of the idea that man's surroundings have shaped his development, and sought to trace its influence in the biographies of former generations. Those who stressed the importance of heredity were driven in the same di-

rection, for by the nature of things it was difficult to study the genetic transmission of human characteristics in any other way (**79**, p. 67).

Since the records of ordinary men have seldom been preserved by history, most of the work that was done dealt with the extraordinary—with the "great" men of former times—and sought in one way or another to deal with the question: What is the true nature of *eminence?* Thus, Francis Galton, in his pioneering work *Hereditary Genius*, published in 1869, wrote: "Is reputation a fair test of natural ability? It is the only one I can employ . . . " (**37**, p. 37).[5] Galton's investigations led him to conclude that "men who are gifted with high abilities . . . easily rise through all the obstacles caused by inferiority of social rank," but that those who have all the social advantages cannot succeed "unless they are endowed with high natural gifts" (**37**, p. 43). Others, of course, have come to different conclusions (**2**, **34**, **76**), but perforce have turned to the same kind of biographical evidence to "prove" their points. The methods developed by Galton and those who followed him in the study of eminent persons illustrate well the use that psychologists have made of biography.

In his studies, Galton drew upon biographical collections (Foss's *Lives of the Judges*, Lord Brougham's *Statesmen of the Reign of George III*, Middleton's *Biographia Evangelica*, and other "ordinary small biographical dictionaries" which were themselves compilations of much biographical writing), and also, in his own words, "very many biographies" of individuals draw from the fields of politics, science, philosophy, and the arts (**37**, pp. 57, 106–107, 259, 324). In a similar way, William James in "Great Men and Their Environment" and "The Importance of Individuals" and Frederick A. Woods in *Mental and Moral Heredity in Royalty* and *The Influence of Monarchs* extracted evidence from biographical works, and reached conclusions that, in general, followed Galton's assertion that heredity was the dominant factor producing human eminence (**46**, **87**, **88**). Charles B. Davenport's *Naval Officers: Their Heredity and Development*, a practical search for an accurate method of selecting future naval officers on the basis of "juvenile promise," made wide use of general compilations like *Who's Who*, Burke's *Peerage*, and the *Encyclopedia Britannica*, and of individual biographies whenever they were available (**25**, p. 3).

Investigators who rejected the *conclusions* of Galton and his followers nonetheless worked with the same kind of sources. John Fiske's reply to James's "Great Men," called "Sociology and Hero-Worship: An Evolutionist's Reply to Dr. James" (**34**), and a similar rebuttal by Grant Allen (**2**) drew from biographical information.

These studies were all more or less haphazard in their choice of evidence but with the burgeoning of comprehensive biographical dictionaries in the late nineteenth century more scientific statistical investigations became possible. This type of research was carried on by historians and biographers as well as by psychologists (Henry Cabot Lodge's "Distribution of Ability in the U. S." [**55**] was perhaps the earliest work of this

[5] Over fifty years of research did not provide a better answer to Galton's question than his own. In 1926 Catharine C. Cox wrote in *The Early Mental Traits of Three Hundred Geniuses:* "Since the problem of the early mental development of great men cannot be approached directly, because of the lack of an objective criterion of greatness, it is approached indirectly through the study of eminent men . . . " (**23**, p. 31).

kind), but the psychologists developed the method most fully. James McKeen Cattell, who took an intermediate position in the debate over the importance of nature and nurture, compiled his well-known list of 1,000 eminent men by computing the amount of space devoted to the individuals described in six biographical dictionaries (**18**). The use to which Havelock Ellis placed the British *Dictionary of National Biography* and the "over three hundred biographies of individuals" which he read in order to supplement that massive collection in writing his *Study of British Genius* (**31**) was essentially similar to Cattell's, although slightly more subjective. And even a much less scientific investigator, Cesare Lombroso, whose *The Man of Genius* (**56**) attempted to demonstrate a close relationship between genius and insanity, based most of what he had to say on information obtained from life histories.

The study of genius which made the most comprehensive use of biography was Catharine C. Cox's *The Early Mental Traits of Three Hundred Geniuses* (**23**). Selecting her subjects from Cattell's list, she sought facts indicative of the IQ's of these men in their personal writings and in the best available biographies. The result was both detailed and convincing, drawn from several thousand biographical works.

Many other investigators of the nature of genius have also analyzed life histories. Evelyn Raskin, in her comparison of literary and scientific leaders (**71**), studied *Lippincott's Biographical Dictionary*, the *Encyclopedia Britannica* and the biographical evidence contained in numerous histories of science and of literature. Similarly, Edwin L. Clarke's *American Men of Letters: Their Nature and Nurture* (**22**) and Cora S. Castle's *A Statistical Study of Eminent Women* (**17**) deserve mention in any list of works of this type, as do also the more recent *Studies in Genius* by Walter G. Bowerman (**14**), Mapheus Smith's "Racial Origins of Eminent Personages" (**77**), and R. K. White's "The Variability of Genius" (**83**).

Another use to which biography has been put by psychologists has stemmed from the work of the psychoanalysts. Psychoanalytical life histories, already described as an aspect of psychology's influence on biography, also illustrate the reverse; in preparing case studies of historical characters, psychoanalysts have inevitably drawn upon standard biographies. These case studies[6] are, however, essentially unlike psychoanalytical biographies. They are more methodological demonstrations than attempts to explain the subjects considered. They are meant to illustrate psychoanalytical principles, whereas psychoanalytical biographies use the principles only to explain the personalities of the characters they describe.

In the field of social psychology, some writers have made use of biography. Hadley Cantril, in his *Psychology of Social Movements* (**16**), studied works on Hitler to explain Nazism, and on Father Divine to explain that individual's cult. A similar but far more elaborate example of this type of work is offered by Gustave Gilbert's *Psychology of Dictatorship* (**38**). Although most of Gilbert's material was drawn from his interviews with captured Nazi leaders, his chapter on Hitler depended considerably upon biographies of *Der Führer*.

Other psychologists have turned to biography in an effort to discover patterns of human development.

[6] The works of this type are so numerous that it would be fruitless to attempt even an abbreviated listing of them here.

Charlotte Bühler's work with over two hundred life histories led to her conclusion that most lives can be divided into five stages: youth, period of trials, maturity, achievement, and decline (**73**). Similarly, Lehman's many studies of the years of peak performance in various fields of specialization illustrate an adaptation of biography to psychological purposes (see especially **54**).

Biography has also been developed as a teaching technique. R. M. Elliott's course in "Biographical Psychology" at the University of Minnesota, in which each student studies biographical works about some person and writes a paper on "the success and lapses of insight with which the development of the human being is portrayed,"[7] has long been well known. Gordon W. Allport at one time adopted William E. Leonard's revealing autobiography *The Locomotive God* as a text for his course in psychology at Harvard (**3**), using it as "a hatrack for an entire course of study."[8] At least one other psychologist, Henry Clay Smith, then at the University of Vermont, has followed Allport's lead.

Psychologists have been neither as free nor as extreme in commenting on biography as biographers have in commenting on psychology, but they have expressed a considerable range of opinions. In general, those who have used biographies in their research have assumed the factual information contained in them to be reasonably accurate, and have tried to follow the most "authoritative" authors and to cross-check their evidence whenever possible. But they have made numerous technical criticisms. Biographers seldom realize, Havelock Ellis argued, that their discipline demands "something more than pure literary aptitudes." His study of the *Dictionary of National Biography* led to him complain of a general lack of method among life writers. Often such important factors as whether or not a subject had ever married, his precise position in his family, and even so elementary a matter as a competent physical description were not to be found (**31**, pp. 16–18). Cox has warned that the average biographer is a synthesizer, not an analyzer, from whose work the perceptive psychologist must strip every vestige of interpretation (**23**, p. 24), and Evelyn Raskin has noticed that biographies of scientists fail to place enough emphasis on the personalities of the individuals they describe (**71**, p. 24). Alfred M. Tozzer has complained of the way many biographers distort or ignore the simplest laws of heredity in a frantic "cytological search for famous chromosomes" that will "explain" a man's fame in terms of the characteristics of his ancestors (**80**, p. 422).

One of the strongest criticisms of biography was made by R. K. White after completing a study of the versatility of geniuses based on Cox's data:

> Biographical data of the usual type are by their very nature relatively unreliable. It is exasperating, for one who holds before himself a high standard of scientific precision, to be eternally conscious that his rock-bottom facts are not scientifically precise. On the contrary, they are often snap judgments, made by untrained observers, often by prejudiced observers, without reference to established norms (**83**, p. 481).

Finally, the comment of Lewis M. Terman should be mentioned. Terman decried "the utter inability of a majority of otherwise competent biographical writers to appraise and interpret . . . the early mental development of their subjects." He at-

[7] Elliott, R. M. Personal communication, January 7, 1954.

[8] Allport, G. W. Personal communication, December 30, 1953.

tributed this to a lack of "detailed acquaintance with the age norms of childhood performance." He illustrated his point by referring to Karl Pearson's life of Francis Galton, in which the author claimed that as a child Galton had demonstrated no significant signs of precocity, despite the fact that his book described youthful accomplishments that led Terman to believe that Galton's IQ was "not far from 200" (**23**, p. v).

In general, biography has not made the impact upon psychology which that relatively youthful discipline has made upon it. The enthusiasm that was generated by Galton's *Hereditary Genius* has pretty generally been dissipated by time. In other psychological areas, biography has been used frequently, but in rather isolated special cases (**16**, **38**), or in experiments that have attracted the interests of single investigators (**54**).

But in addition to what biography can offer to the modern psychologist by way of source material, it can also provide him with many brilliant examples of character portrayal which demonstrate the importance of literary ability in any work seeking to explain that complex organism, man.

After completing his exhaustive study of fifty men of college age, Henry A. Murray saw this point clearly:

> Any conceptual formulation of man's experience . . . must necessarily do violence to human feelings. . . . This will be so because it is the substitution of heartless, denotative referential symbols for the moving immediacy of living. By employing such a scheme a person's vital movements, once warm and passionately felt, become transformed into a cruelly commonplace formula, which dispossesses them of unique value. . . . The artist's representation of an experience, on the other hand, is a re-invocation of the original . . . equally immediate, exciting and intense (**65**, pp. 17–18).

Murray therefore sacrificed some of the scientific objectivity of his studies on the altar of literary style. The same point has been made by Allport: "The greatest failing of the psychologist at the present time is his inability to prove what he knows to be true" (**4**, p. 9). Murray and Allport were suggesting that psychologists can learn from creative literature, but biography, with its stress on both artistry *and* accuracy, can, in its finest form, be a still better teacher.

In conclusion, it seems clear that biography and psychology have always been related disciplines. Early intuitive interrelationships have been supplemented in recent times by more conscious and more scientific efforts in each field to profit from the work of the other. Psychological knowledge has aided modern biographers in understanding their subjects, and biographies have been used by psychologists interested in the study of genius, heredity, social movements, and many other topics. Yet there remains room for further development. Scientific psychological methods ought to be thoroughly investigated by biographers, while psychologists should more often turn to biography as providing a source of psychological data.

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CLARIFICATION OF AN AMBIGUITY IN HULL'S *PRINCIPLES OF BEHAVIOR*

J. J. CLANCY, L. THOMAS CLIFFORD, AND ALLEN D. CALVIN
Michigan State College

In *Principles of Behavior* (2), Hull attempted to set forth a rigorous hypothetical-deductive system. This tremendous step forward in psychological system building has inspired more research since its publication ten years ago than any other book in the area of experimental psychology. For this reason any incongruity or ambiguity in one of the basic postulates or corollaries calls for prompt clarification.

An example of such an ambiguity occurs in Corollary VII on page 154. This corollary reads as follows: "Equal differences in the delays of reinforcement of two competing acts lead with equal practice to a lower per cent of preferential choices of the act associated with the shorter delay when the two delays are large (i.e., when the ratio is relatively coarse) than when they are small (i.e., when the ratio is finer)."

Here Hull is obviously using the terms "coarse" and "fine" to mean that the larger the fraction the coarser the ratio, i.e., 1:2 would be coarser than 1:3. However, in a preceding statement Hull notes, "Let us take, for example, the same theoretical arrangements assumed above, with the exception that the delays are of 30 and 90 seconds respectively, which gives the organism the relatively coarse ratio of 1 to 3 instead of 1 to 2, as previously" (p. 152). Again in his summary he reiterates, "Moreover, it follows from these same principles that the point of maximum ease of discrimination of a coarse ratio, such as 1 to 3, will appear at smaller absolute values than will be the case with a fine ratio, such as 1 to 2" (p. 159). This usage of coarse and fine stands, of course, in direct contradiction to the usage of these terms in the corollary cited above.

We are thus faced with an ambiguous situation, which can be resolved in two possible ways. We can reverse the words "large" and "small" and change the corollary to read: Equal differences in the delays of reinforcement of two competing acts lead with equal practice to a lower per cent of preferential choices of the act associated with the shorter delay when the two delays are small (i.e., when the ratio is relatively coarse) than when they are large (i.e., when the ratio is finer); or we can reverse the words "coarse" and "fine" so that the corollary reads: Equal differences in the delays of reinforcement of two competing acts lead with equal practice to a lower per cent of preferential choices of the act associated with the shorter delay when the two delays are large (i.e., when the ratio is relatively fine) than when they are small (i.e., when the ratio is coarser).

These two alternatives are diametrically opposed in meaning. If we interchange the words large and small, as we did in the first alternative, the corollary states that there will be a lower percentage of choices when the two delays are small, i.e., when the ratio is relatively coarse. If we interchange the words coarse and fine, as we did in the second alternative, the corollary states that there will be a lower percentage of choices when the two delays are large, i.e., when the ratio is relatively fine.

Fortunately Hull provides us with the answer as to which is the proper alternative to choose. He states that this corollary finds empirical confirmation in a study by Anderson (1). An examination of Anderson's data, which Hull presents, indicates that the second alternative is the one that is in accord with these findings. Thus it seems apparent that this corollary of Hull's should be corrected to read: Equal differences in the delays of reinforcement of two competing acts lead with equal practice to a lower per cent of preferential choices of the act associated with the shorter delay when the two delays are large (i.e., when the ratio is relatively fine) than when they are small (i.e., when the ratio is coarser).

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A REJOINDER ON ONE-TAILED TESTS

LYLE V. JONES
University of Chicago

In a recent issue of this journal, Burke (**1**) criticizes earlier discussions of one-tailed and two-tailed tests (**3**, **4**, **5**) and suggests need for caution in the application of one-tailed statistical tests to psychological research designs. The writer is in accord with several implications of Burke's note. As is true for most statistical designs, abuses would be reduced markedly were the test model completely specified and justified in terms of the purpose of investigation, before data are viewed by the investigator. To be guided by the data in the specification of hypotheses and statistical tests is a grave breach of the rules of experimental verification (cf. **6**).

The argument presented by Burke, however, is more than a plea for careful consideration of the choice of test models for given experimental problems. It is stated that the selection of a one-tailed test model requires that an investigator be willing to "publicly defend the proposition . . . of no difference" if results actually show a large difference in the direction opposite to that predicted (**1**, p. 387). The proposition appears indefensible, since it demands arguing that a particular observed difference, no matter how large, is only a sampling departure from zero. If accepted, Burke's argument should lead to universal avoidance of one-tailed tests.

Consider the following experimental problems, selected for simplicity as single variable designs: (*a*) On the basis of a certain behavioral theory, we might predict that an experimental condition imposed upon subjects in the population under study would raise the mean level of performance on a given task. The theory provides a prediction (an alternative hypothesis) that the mean for the experimental group population will exceed the mean for the control group population. The hypothesis under test is that the mean for the experimental population is the same as or less than that for the control population. We should like a statistical test that will yield a decision: either the data are consistent with the hypothesis under test, or we reject the hypothesis in favor of the alternative. (*b*) In a field of applied psychology a new diagnostic technique is developed and is to be adopted if, and only if, we are confident that it is better than the current technique which would be replaced. Assuming the availability of a suitable criterion, the two techniques are applied to comparable samples or to the same sample; interest resides in the extent to which the parametric proportion of successful predictions using the new technique exceeds that using the old. A statistical test is to supply a decision: either the new technique is no more adequate than the old, or the new technique is more adequate.

For the class of problems illustrated by these two examples, the hypothesis under test is not simply one of no difference. We wish to test the hypothesis that the algebraic difference between parametric mean performance under experimental and control conditions is zero *or negative* against the alternative hypothesis

that the difference is positive. It is meant to stress this formulation of the one-tailed statistical test with the greatest possible emphasis. With this formulation, it is apparent that acceptance of the hypothesis under test does not demand defense of a proposition of no difference; the observed difference, whether negative, zero, or slightly positive, simply does not allow acceptance of the alternative hypothesis at the level of stringency (values of α and β) chosen for the test.

In a footnote, Burke (**1**, p. 385) criticizes this formulation as it appeared earlier (**4**, p. 45) on the grounds that it confuses hypotheses to be tested with hypotheses to be guarded against as alternatives. To the contrary, this statement of the problem clarifies the nature of the hypothesis under test. The hypothesis to be tested is[1]

$$H_0: \mu_e - \mu_c \leqq 0,$$

where μ_e is the population mean for the experimental condition, μ_c is the population mean for the control condition, and the experimental prediction yields an alternative hypothesis,

$$H_1: \mu_e - \mu_c > 0.$$

Burke's primary argument seems to rest upon the contention that "there is, under the single-tailed rule, no safeguard whatsoever against occasional large and serious errors when the difference is in the unexpected direction" (**1**, p. 385). Our formulation of the hypothesis under test completely resolves this difficulty, for no error is committed when that hypothesis is accepted on the basis of a large observed difference in the unexpected direction. The event is one of a class of events consistent with the hypothesis tested.

If one were to retain the alternative hypothesis, H_1, above, and to adopt a two-tailed statistical test, accepting H_1 when there were observed large differences between the means *in either direction*, his position would be unenviable. For, following the rules of his test, he would have to reject the hypothesis under test in favor of the alternative, even though an observed difference, $\mu_e - \mu_c$, was a substantial negative value.

The remaining discussion by Burke consists of pragmatic arguments against the adoption of one-tailed tests. The arguments appear valid only under the assumption that every application of the one-tailed test is an abuse of experimental methodology. Certainly, if (*a*) the test model is specified completely (including specification of the confidence level to be adopted) before the data are gathered, and if (*b*) the purpose of the test is only to determine whether a particular directional prediction is supported by the data, then the one-tailed test not only is appropriate, but it is in error to use a two-tailed test model.

[1] An equivalent formulation of the one-tailed test model, of which the writer was unaware at the time of his earlier note, is that proposed by Dixon and Massey (**2**, pp. 100–104).

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FURTHER REMARKS ON ONE-TAILED TESTS[1]

C. J. BURKE
Indiana University

In the discussion of one-tailed tests there are a number of issues which have not been clearly separated. An effort will here be made to separate them.

TWO TYPES OF MODELS

Psychologists have used two different types of models in interpreting their data: statistical models and specifically psychological models. The two types have been employed in different ways and for entirely different purposes. Statistical models have been used to determine whether data are intrinsically interesting or, in other words, whether the data indicate relationships between psychological variables. Psychological models have been utilized in attempts to organize the knowledge attained in various areas of the field.

Early uses of statistical methods mixed the psychological and statistical models. The substantive biases of the experimenter led him to seek out, with each set of data, those statistical techniques which supported his views and to neglect the techniques which did not. Gradually, it was recognized that the function of the statistical model was prior to and separate from the function of the substantive model. Cognizant of the fallibility of their data and the greater fallibility of their theories, psychologists developed the view that data should be interpreted without the intrusion of the biases, however well founded, of the experimenter and accordingly accepted certain statistical procedures as conventions. The advocates of one-tailed tests would take us a large step backward, for they openly favor the mixing of the statistical and substantive models.

Any psychological theory worth bothering with will generate a number of predictions which can be directly checked against experimental data. Such direct determination of the consistency of data with the model is the only proper test of a psychological theory. In a check of this kind, there is a point by point verification of the theory, and strictly speaking, no statistical model is necessary. This does not mean that there should be no statistical interpretation of the data; it is usually wise to make the ordinary statistical interpretation prior to the theoretical check—to determine whether the data show enough to warrant a theoretical analysis. If, in any given experiment, a theory provides only a directional prediction, there seems to be nothing wrong with our traditional procedure of first establishing the presence of a difference between groups and subsequently noting its direction. A statistical model should enter directly into the verification of a substantive theory only when it is based on a null hypothesis with respect to the residuals of the data from theoretical predictions.

A statistical model can be viewed as a technique for assessing the reliability of an experiment without repeating it. The question answered by a statistical test is whether or

[1] The writer thanks his colleagues, A. M. Binder and W. K. Estes, for their suggestions after reading an earlier draft of this reply.

not the sample obtained is a member of a certain class of samples. This class is usually defined by the null hypothesis to be tested and by the conditions under which the experiment would be repeated, if repetition were undertaken; frequently certain supplementary parametric assumptions must be made. Since an experiment can be repeated without any appeal to a body of substantive theory, no such appeal need ever be made for purposes of statistical analysis. Experiments are often designed from theoretical considerations, and the conditions of repetition may, therefore, be dictated by a substantive model. In some analysis of variance designs there may be freedom in the choice of an error term. This freedom indicates that the single experiment which has been performed, even in conjunction with a null hypothesis, does not define a unique class of repetitions. When this is the case, appeal to substantive theory may lead to the choice of a proper error term, and this appeal may seem to involve a direct intrusion of substantive theory upon the statistical interpretation. As we have put the matter, however, it is readily seen that in this instance the theory does no more than define the conditions under which the experiment would be repeated.

The One-Tailed Test in Theoretical Statistics

Properly described, the concept of a one-tailed test is clear and free from any objection on mathematical grounds. There is no disagreement on this point. Disagreement on what constitutes a proper description of a one-tailed test centers specifically on the nature of the hypothesis that is tested. Jones has emphasized his conviction that the hypothesis of a nonpositive difference is tested against the alternative of a positive difference. I have stated that a null hypothesis is tested with safeguards against only a subset of the possible alternatives.

As it now stands, the mathematical theory of testing statistical hypotheses requires that the hypothesis under test lead to the calculation of a level of confidence. (Until the onslaught of the one-tailers, books in psychological statistics cautioned us to test only "exact" hypotheses.) If we ask Jones to exhibit his calculation of the level of confidence in a one-tailed test, we shall probably discover that he bases the calculation on the distribution obtained under the null hypothesis. Then, according to the usual statistical logic, he is in fact testing the null hypothesis against selected alternatives. The comments of my earlier note were made within this framework.

An easy modification of statistical logic can make the one-tailed test of a nonpositive difference tenable. Since the probability that he rejects the hypothesis of a nonpositive difference when it is true can, in his procedure, never exceed the level of confidence calculated under the null hypothesis, Jones can declare that he has a bound on his level of confidence even though he does not know its precise value. Utilizing this bound, he can set up consistent rules for one-tailed interpretation of the hypothesis he has stated.

The two paragraphs above present what seem to be the only two positions open to Jones. Whichever he accepts, I am in complete accord with him so far as mathematical statistics goes, for both are clear and statistically defensible. Yet his succinct assertion that I advocate universal

avoidance of one-tailed tests is correct. The locus of disagreement is to be found in practice, not in theory.

Applied Problems

In applied problems like Jones's example of the drugs, it is often true that certain practices or routines will be changed if, and only if, an experiment yields a reliable difference *in a specified direction*—if the new is reliably better than the old. For problems like this, Jones maintains that the two-tailed test is inappropriate. Verbally his argument proceeds well, but before we accept it, let us note that problems of this kind have never given any difficulty—we have always understood what we did and why. Let Jones use his one-tailed test at the 5 per cent confidence level to analyze data from a large number of such experiments. We shall analyze the same data using a two-tailed test of the null hypothesis but permitting ourselves a luxuriously loose 10 per cent confidence level. Being sensible men, we shall not advocate a change to the new routine if the old one proves reliably superior. Unless we wish to differentiate a null from a negative difference (we might, for example, consider a replication necessary unless the null hypothesis were rejected), the two procedures will obviously lead to the same recommendations with every set of data. Thus, they have identical consequences for the immediate practical decision.

For myself, I do not much care whether one uses a one-tailed or two-tailed model in such applications. Since the choice of a level of confidence is arbitrary and since identical decisions will always be made, we have complete equivalence between the one-tailed model and a doubled level of confidence in our traditional procedure. It would be wrong for either Jones or me to pretend that compelling reasons exist for preferring one of these ways of talking about applied experiments over the other. Any appeal to the risks of the two kinds of errors is little more than an uninteresting parlor game. Since no gain results, there seems to be insufficient reason for changing the traditional two-tailed interpretation, but this is not a very strong objection. In my earlier note, I conceded the admissibility of the one-tailed test for the experimenter's own laboratory planning; I am quite willing to add the applied experiment to the list.

The Public Literature

There do seem to be compelling reasons why the use of one-tailed tests in our permanent reports must be rejected. These reasons are of two kinds: one kind residing in the nature of the scientific enterprise and the functions of the experimental literature, and the other associated with the debasing effect upon the literature that is almost certain to result from adopting one-tailed tests.

Experimental scientists must have for data a permanent respect that transcends their passing interest in the stories they make up about their data. Science is a public enterprise. These are statements to which most experimental scientists would subscribe. One-tailed tests violate both the spirit and the letter of these statements. Interest in whether a relationship is found between two variables in a given experiment is seldom confined to those who share the theoretical preconceptions of the experimenter, and the right to discuss experimental data in relation to a particular theory does not remove the obligation to interpret the experiment according to rules

that can be accepted by the reader who rejects your views as well as by the reader who shares them. Our experience has shown that experimental results can be viable for years after they are first reported even when the experiments are designed on the basis of moribund theoretical considerations. Let us consider our obligation to future psychologists who might not be able to understand why we were so naive in 1954 as to predict *that* direction for our experimental results. Why should we put needless difficulties in the way of any reader, present or future?

Why, particularly, when the change over to one-tailed test is almost certain to gain nothing and lose much? Jones asserts that my dire forecasts on the effects of one-tailed tests on our literature are predicated on the assumption that every one-tailed test is a misuse. Here he has missed the fundamental point of my argument. As in applied experiments, there is pragmatic equivalence between using the one-tailed test and doubling the level of confidence in the two-tailed test, for if the discrepancy is in the expected direction, the power of these two tests is identical, and experiments should be, and usually are, set up with power as a foremost consideration.

Most psychologists would resist the general adoption of the 10 per cent level for rejecting hypotheses. Such a low level of aspiration would lead to a decreased number of subjects per experiment and thus accomplish a gradual attrition in the soundness of our reported conclusions. This is precisely what will happen if there is general, and editorial, acceptance of the one-tailed test.

Concluding Remarks

A diminution in the quality of our psychological literature may be of little moment to the theoretical statistician but must be of grave concern to the experimental psychologist. Once they have the assurance of the mathematical statistician that the procedure is logically defensible, experimenters seem so eager to employ one-tailed tests that, sociologically speaking, I have lost the argument almost before it has been joined. Of course, the procedure is mathematically sound, but psychologists are mistaken if they believe the mathematical statistician can speak with special authority in the matter. The decision is for us alone; it is for us to ponder whether, in the interpretation of our data, the one-tailed test is wise. We are drifting into an unfortunate decision, and I, for one, wish to enter a plea for a little less methodology and a little more wisdom. We expound a distinction between experimental hypotheses and statistical hypotheses to our beginning students; let us maintain the distinction for ourselves.

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WOODWORTH, R. S., & SCHLOSBERG, H. *Experimental psychology.* (Rev. Ed.) New York: Holt, 1954. Pp. xi+948. $8.95.

It is a pleasure indeed to note the appearance of this revised edition of one of psychology's classics, Woodworth's *Experimental Psychology*, which has graced the shelves of every graduate student for a generation. In a real sense the tradition is even longer, for the spirit of this revision is very much the spirit of Ladd and Woodworth's *Physiological Psychology* which an older generation will remember (and perhaps that of the still older Ladd, which this writer willingly confesses he does not remember!). The revised edition is a concretely objective account of the principal facts discovered by psychologists in the areas of sensation and perception, learning, and the emotions, with frequent appeal to established physiological mechanisms for explanation.

It should be emphasized at once that this is truly a revision and not just a freshened-up second edition. While the range of material, and even many of the chapter headings correspond closely to the Woodworth book, the treatment of some topics has undergone very substantial change. Learning, in particular, has been almost completely reorganized. Three of the old chapters have disappeared: Retention, Memory for Form, and Practice and Skill. Three new chapters have been added: Introductory Survey, Discrimination Learning, and Motivation in Learning and Performance. New names appear: Hull, Spence, Skinner, Tolman, Hilgard, Hovland, while older names take second place, G. E. Müller, Ebbinghaus, Pavlov, and Carr.

Changes in other sections of the book are less drastic. Feeling, Experimental Esthetics, and Reading are not given separate treatment, and the chapters on Problem Solving and Thinking are combined into one shorter chapter. The subject of emotion is given a new, substantially more physiological, treatment. Of the sensory chapters, Vision, in particular, reflects the renewed emphasis which the war gave to this side of experimental psychology.

It is always tempting to a reviewer to seek some measure of the amount of change which has been made in revising a book. In this case, a list was made of the most frequently cited (indexed) authors. Highest ten in the 1938 book were, in order, G. E. Müller, Wundt, Ebbinghaus, Köhler, Cattell, Dodge, Judd, Thorndike, Pavlov, and E. H. Weber. Of these, only Thorndike survives in the 1954 list. The list now runs Hull, Boring, Hilgard, Thorndike, Stevens, Lashley, Graham, Wever, Hovland, Tolman. Quite a change!

The present book is roughly 25 per cent larger, yet less attention is given to any one person. This makes doubly surprising the fact that Hull is cited almost twice as frequently as any single person in the 1938 edition. If anything is suggested by this tabulation, it is that there is considerably more dependence in this book on secondary sources. The frequent references to Hull, Boring, and Hilgard, for instance, are rarely to original papers, but much more to their organized judgments about the facts in a given experimental field.

The critical review of a book of this kind is not easy. It contains too many detailed facts. The perspectives are rarely global. There is the added uncertainty of which of the two authors has seen a problem in a given way. It would be easy, therefore, to add some comment at this stage recommending this book for its undoubted excellence, and advising teachers and librarians that it merits their special attention. Both comments are true. But the real test of these propositions lies in the popularity which *Experimental Psychology* has enjoyed over the past 16 years, and in whatever degree of acceptance the revised edition may find in the next decade. It is of much more interest to see the kind of reflection which the book affords us of the contemporary state of affairs in experimental psychology.

1. The strongest impression of this reviewer is the striking unevenness in the development of various parts of the field. Some of this may proceed from the very catholicity of interest on the part of the authors. Or it may be that there are larger gaps between older and younger generations of psychologists than some of us would care to admit. Nevertheless, the diversity in approach, in sophistication as to method, and in the formalization of results strikes the reader repeatedly in trying to read such a book as this from front to back. The first three chapters (skipping an Introduction) on Reaction Time, Association, and Attention present almost sheer anachronism. The problems, objectively conceived, certainly are problems with which the experimental psychologist wants to deal, but scarcely in this intellectual framework. But the authors' trouble was obviously how to recast the older facts to fit them to a currently acceptable pattern. Where, for instance, is the body of modern evidence to which old facts about set and attitude and the selectivity of attention should be related?

To take another contrast, there is the curious difference between the field of learning on one hand and the sensory fields on the other. There is no question that great changes have taken place in the field of learning. To this the revised edition is ample testimony. Neither "reinforcement" nor the "law of effect" can be found in the 1938 index; their omission in the 1954 book would be unthinkable. And yet the field of learning is full of doctrinal controversy. Is there one, or are there two kinds of conditioning? Can we depend on the shape of the generalization gradient? What about the conflicting views of the nature of extinction? In short, theory building and the effort at semantic clarification seem to be the only analytic tools at hand. In contrast, nothing frightens the sensory psychologist so much as a bit of theory. His experiments are brutally detailed; their plan, entirely parametric. For explanation, he takes flight into physiology. Is this, rather than theory building, the syndrome of a mature science? Certainly, as presented in the revised edition, these two areas of interest follow an utterly different pattern, and the difference is puzzling indeed.

2. Not unrelated to the unevenness in development and in method is the lack of an overview of what psychology's problems are, where they come from, and to what consequences they lead. Like Topsy, most of the questions in this book "just growed." For the expert or for the technician this deficiency is unimportant. He does not go to a factual compendium such as this to answer the question

about why, as a psychologist, he wants to know this or that. But it is not entirely unimportant for the graduate student who reads a treatise of this kind. He is more likely to be forming some notion of the way in which an experimental psychologist regards human experience and human behavior. One thing this book makes clear—the methods to be followed are experimental. Concrete methods are repeatedly detailed. But often the reader, at least this reader, gets the impression that we drift into experimental work on an issue without keeping our original objectives in mind.

Let us take the question of the GSR and emotion. Very roughly the conclusion seems to be that the GSR is probably related to something called the level of activation. Or at least the GSR reflects changes in the level of activation. But the GSR does not have much to do with what has traditionally been called emotion and that was the broad class of behavior with which the discussion started. Perhaps the term emotion is just another dead dodo, but the reader may feel that the GSR is a Trojan horse that takes the field without a battle. Too often, the discussion slips from a problem to a method, and from the method to controversy about the method. This latter is not always psychology.

3. When psychology deals with questions asked as often for methodological as for substantial reasons, systematic comparisons of the findings in related fields are likely to be lacking. For instance, a great deal of the discussion of vision hinges around stimulation with short flashes of light. The parallel experiments on the temporal course of stimulation in other sense modalities come in for rather little discussion and the possibility of a common underlying mechanism does not come up. The same kind of question is, however, recognized with regard to adaptation—and remains unanswered. Perhaps the contrast is most striking if one recalls the almost overelaborate treatment of interrelated experiments in Koffka's *Principles of Gestalt Psychology*, or the parallel effort in Hull's *Principles of Behavior*. It is indeed a wise man who can steer the way between the Scylla of theoretical vacuity and the Charybdis of a mere taxonomy. This reviewer would argue that the authors have sailed a bit too close to the whirlpool.

It would not be proper to close this review without a word of appreciation for the real scholarship that lies behind this revised edition, quite as much as it did in the case of the 1938 *Experimental Psychology*. Everyone who sits down with the book, though he will miss some details of topics near to his own interests, may as often find the discussion cogent and complete. But rarely, very rarely, will some mention of an important line of research be entirely lacking. Within certain limits as to subject matter, there is amazingly little that has not been at least considered by the able authors. This in itself is no mean achievement.

EDWIN B. NEWMAN
Harvard University

WEITZENHOFFER. A. M. *Hypnotism: an objective study in suggestibility.* New York: Wiley, 1953. Pp. xvi+380. $6.00.

A good deal of experimental work in the fields of suggestibility and hypnosis has been done since Hull published *Hypnosis and Suggestibility: An Experimental Approach*. A number of books have been published in this field too, but they have usually

been of a speculative character or have dealt only with limited aspects of the field. *Hypnotism: An Objective Study in Suggestibility* is the first real attempt to pull together all the available experimental material, to assess it critically, and to draw whatever conclusions may be justified on the basis of the findings. The attempt was well worth making, and the scientific care and objectivity of the author have succeeded in making this an indispensable book for anyone interested in the theory and practice of hypnosis, or doing research in this field.

The book is divided into four parts, the first of which gives a general orientation and deals with such topics as the criteria and stages of hypnosis, tests of suggestibility, and so forth. The second part deals with experimental studies of the *intrinsic* and part 3 deals with the *extrinsic* characteristics of suggestibility and hypnosis. This differentiation is not clearly defined by the author. By intrinsic he seems to mean such things as the factors influencing suggestibility, the interrelationship of hypnosis and suggestibility, and "animal hypnosis." By extrinsic he seems to mean the effects of suggestion and hypnosis on sensory and perceptual functions, on memory and learning, on organic functions, and so forth. Some of the chapters included in one part, however, would seem to fall more easily into the other, if this be indeed a correct interpretation of the author's wish; altogether this division does not seem to be a very happy device. The last part deals with a review of theoretical formulations and presents the author's own theory.

Weitzenhoffer accepts the division of suggestibility into primary, secondary, and tertiary, and builds his account around these notions of independent factors. In doing this, however, he is not always consistent. Thus, on page 33 he quotes with approval data showing that neurotics are more suggestible on a test of primary suggestibility; a few lines later he quotes another investigator's results showing that neurotics are more suggestible on a test of secondary suggestibility. Weitzenhoffer concludes that the latter *confirms* the earlier work. Now if primary and secondary suggestibility are indeed independent factors, a test of one type cannot itself confirm results obtained from a test of the other type. This tendency to have his cake and eat it too (i.e., to regard two factors as independent, but yet to assume that tests of one type can confirm results obtained by tests of the other) occurs several times throughout the book and may be misleading to readers not familiar with the meaning of orthogonal factors.

On most occasions, Weitzenhoffer's conclusions from the evidence are unexceptionable and will be accepted by most students of the subject. Occasionally, however, he seems to miss the mark. Thus, in his summary of the literature dealing with the relationship between neurosis and suggestibility he ascribes the contradictions found to the fact that differences are observed when psychiatric diagnoses are used, but not when neuroticism inventories are used. A different summary might be that differences are found when severe neurotics are compared with normal people, but not when we restrict the range so much that only normals are included in the sample.

The documentation of the book is very thorough indeed, the bibliography running to over five hundred titles, all well selected. The reviewer has been able to find only one major

omission that would alter the conclusions Weitzenhoffer draws. In his discussion of hypnosis and dissociation (p. 99), Weitzenhoffer quotes the work of Mitchell, which is negative, but not that of Nagge, which is positive. Since it is derived from a larger number of cases, the latter appears more trustworthy and would have altered the conclusion drawn if it had been considered. This is important, for it determines the author's attitude to the dissociation hypothesis.

On much disputed points like the existence of animal hypnosis, or the possibility of inducing antisocial acts under hypnosis, the author sums up with impartiality and bases his conclusion entirely on the evidence without showing any tendency to try to make the evidence agree with preformed conclusions. This tendency to follow the facts rather than to try to press them into the service of some general theory makes this quite an unusual book in the field.

The major new contribution of the book lies in the theory advocated by Weitzenhoffer to account for the phenomenon of hypnosis. This is a three-stage theory: "*Hypnosis is the outcome of not one but of several temporally overlapping and interacting processes.*" The two main ones the author considers to be those of *homoaction* and *generalization;* in the later stages of hypnosis a third process, namely a *narrowing of consciousness*, is introduced. "All of these complement each other, and are interwoven, although each appears to have a different mechanism of action and different end results when considered singly." This theory is too complex to be considered in any detail here. It seems to have the usual advantages and disadvantages of theories involving the interaction of several processes, none of which can be specified or measured with any degree of accuracy. It would probably be possible to account for most hypnotic phenomena along the lines of this hypothesis, but it would be very difficult to make predictions which could invalidate it. Another weakness of this theory seems to be that although certain aspects of the theory involve learning, and presumably also drive reduction, the author has nowhere tried to bring his formulation into line with modern learning theory. Such an attempt might have resulted in testable predictions, thus strengthening the acceptability of the theory. In spite of these criticisms, theoretical formulations in this field have generally been so obviously untenable that Weitzenhoffer's formulation stands out as a much more reasonable interpretation of the very complex phenomena under discussion than any alternative theory known to the reviewer. Perhaps it would be unfair to ask for more than that at this stage of progress.

H. J. EYSENCK
University of London

ROE, ANNE. *The making of a scientist.* New York: Dodd, Mead & Co., 1953. Pp. ix+244. $3.75.

Since scientific discoveries have played a central role in initiating the vast changes in human life which have characterized the past two centuries, one cannot doubt the importance of obtaining better understanding of creative investigators. Dr. Roe for several years has been conducting studies of research scientists. She has obtained test data as well as biographical and personal interview data from a considerable number of the leading scientific men of today. Her original reports of her findings have appeared in monograph form and in psychological journals. The

present volume is a summary account of her investigations written for the layman. It contains no new data. The style is informal and easy. However, the reviewer is inclined to believe that most readers who are interested in "the making of a scientist," will find the presentation too elementary. Furthermore, if the reader should become interested in delving further into the characteristics of scientists, this book will not assist him, for he will find in it few references to other publications, including Dr. Roe's own research reports. This is unfortunate, for many psychologists, as well as some general readers, would find a bibliography useful.

The critical reader will doubtless take issue with some of the conclusions which Dr. Roe arrives at from studies of quite small groups of subjects whose representativeness is unknown. However, all in all, Dr. Roe is to be commended for taking the pains to make available to interested laymen an account of her work. Psychologists too seldom undertake to communicate with other than psychologists.

WAYNE DENNIS
Brooklyn College

DOLLARD, JOHN, AULD, FRANK JR., & WHITE, ALICE. *Steps in psychotherapy.* New York: Macmillan, 1953. Pp. ix+222. $3.50.

For reasons which are no longer valid, psychotherapists for over half a century have failed to make available for public inspection the raw data from which their hypotheses and conclusions have been derived. The procedure of relying upon the subjective interpretations of an ego-involved therapist-investigator is gradually being replaced, fortunately, by typescripts of phonographic recordings. Although the present book presents only one verbatim interview from a series of 17 contacts with a patient, with the remaining interviews being abstracted from recordings, the reader gains a definite impression of intimacy not only with the patient but also with the therapist's thinking about the patient. In addition, the verbatim comments of the supervisor appear on the same page in which the interview material is presented. Thus the book presents not only the interpretations and conclusions of the authors about a 40-year-old woman treated by brief, psychoanalytically oriented psychotherapy, but also includes a good deal of the raw data of treatment obtained not only from the therapist, but also from the patient and the supervisor. A final section of the book presents a verbatim account of several tests administered to the patient prior to therapy which were interpreted blindly and independently by two clinical psychologists. The authors' attacks upon the psychological reports are somewhat unconvincing in view of the limited testing and poor rapport with the patient during examination.

Apparently intended as something of a casebook for the earlier book by Dollard and Miller, *Personality and Psychotherapy* (1950), the introductory chapters of *Steps in Psychotherapy* summarize very briefly one learning theory interpretation of the therapeutic process and provide an extremely lucid résumé of the therapeutic principles presented in the earlier book. Although the 17 interviews are intended to illustrate the principles expounded, the relevance of the learning theory is not made particularly discernible in the supervisor's extended comments. The learning theory appears essentially to

be an apologia for the particular brand of brief analytic therapy which is presented. Only in the continued insistence on the therapist's duty to help the patient to "label" his experiences does there seem to be much extension of or departure from the usual procedures of brief, analytic treatment methods.

Regardless of one's point of view toward the brand of therapy advocated, the interested reader is in a position to inspect the supervisor's hypotheses and, to some extent, consider the evidence on which they were based as well as the effect the hypotheses had upon the patient when the therapist followed instructions. It is regrettable that space limitations prevented the authors from publishing more than selected and edited excerpts of the 16 interviews. Such questions as the following indicate the possibilities of using the book as a teaching device, although the answers can't be found in the book because of the limited material presented: (*a*) Was the early diagnosis of sexual maladjustment as the major problem justified, or did it stem from the theoretical bias of the supervisor? (*b*) Was the patient rewarded primarily for discussing sexual material and "punished" for other topics of discussion which might have been important? (*c*) Did the mother and husband play such minor roles in the patient's problems as the authors indicate?

In a charming and disarming sentence the authors "expect to find most helpful the views of those commentators who have themselves published their own uncensored remarks as therapists or supervisors." Although the drive revealed by this cue is commendable in a world where justice *should* reign supreme, the expectation is hardly cricket since it reduces the universe of "qualified" commentators to perhaps five or six persons now living.

This reviewer will rest content with the observation that the edited case material fastens so insistently upon the sexual conflict theme that there is little opportunity for the reader to determine whether any alternative explanations of the patient's symptoms might have been tenable. Such a question does not, of course, detract from the real value of the book in illustrating a particular approach to therapy.

VICTOR C. RAIMY
University of Colorado

PICKFORD, R. W. *Individual differences in colour vision.* London: Routledge & Kegan Paul, Ltd., 1951. Pp. xviii+386. 30s.

This book is primarily a detailed report of seven experiments on color vision carried out over a period of more than eight years with some 1100 observers. The research was designed to develop a diagnostic test of color vision, to help clarify the existing confused classifications of color blindness, and to test the adequacy of the Young-Helmholtz theory of color vision.

These aims were achieved with considerable success. Pickford's four-color test, which was used in his main experiment, turns out to be one which can be readily adapted to the clinical testing situation to give objective diagnostic information. His test data provide an operational basis for defining categories of normality, of color weakness, of anomalies, and of major defectives. His results point strongly toward a theory of color vision much like Houston's modification of the Hering theory, and can be fitted to the Young-Helmholtz theory only with unpar-

simonious additions to the basic trichomatic notion.

Although Pickford himself considers the work he has done as more preliminary than definitive, there are several important contributions which this research has made to the field of color vision testing. Primary among these is the fact that diagnostic categories are clear cut and objectively defined. A statistical procedure is used for defining "deviant" and "color weak" groups as falling respectively in the 1-to-2 and 2-to-3 sigma brackets of the normal distribution, and justifies separating them from the "anomalous" who deviate discontinuously out as far as the 5-to-7 sigma brackets. Protanopes and deuteranopes are distinguished in terms of their much greater color-matching variability and on a basis of their brightness matches. He reports that there are few "pure" dichromats (using the term in its traditional sense), but shows how the "nonpure" dichromats can be separated from the anomalous trichromats.

Another important contribution is the demonstration that individuals who may by some tests be considered normal, may at the same time show an independent weakness in any one of four spectral regions: red, yellow, green, and blue. Pickford shows, however, that there is likely to be some relationship between yellow and blue, or red and green sensitivity.

Of great practical value to those who must give color vision tests in industry or elsewhere are the precautions, described in detail, which show how the clinician should approach an individual to be tested so that the test will not be invalidated. The color defective has learned to use extra cues and all sorts of ruses to get by in his everyday life and frequently is a better examiner of the unwary tester than the tester is of him.

There will be some readers who will be critical of the general experimental technique used. From the standpoint of classical psychophysics, it may appear that the data are skimpy. Typically, one observation by a bracketing procedure was the only basis for scoring a single color-matching judgment, and a single match with each of four colors gave all the diagnostic information obtained for a single observer. There are, however, several factors which make these data considerably more than minimally acceptable. For one thing, the colorimetric technique used in the main experiment is inherently precise, and the bracketing procedure carried out by the experimenter was carefully designed to give a measure of variability as well as one of central tendency. One thing to remember is that the author intended to develop a practical testing procedure so that the whole test could be given in a matter of 10 or 15 minutes. Pickford did run a test-retest reliability series on 36 observers which gave coefficients ranging from .93 to .98 for the more important red and green test colors. The author intends to extend this elaborate and time-consuming preliminary study, and has already organized a survey of at least 20,000 British observers.

Although Pickford's classificational categories are objectively defined, they are specified in terms of the arbitrary scales of his colorimeter. It is unfortunate that he did not report internationally standardized psychophysical specifications (C.I.E.) for his test colors, and that he did not specify his data in these generally comprehensible units for comparative purposes.

Some criticism could be made of

the organization of the book. In general, a chronological sequence of experiments is followed, but it seems cluttered at times with repetition, with the inclusion of too much detail on the subjective reports of isolated individuals, and with introspective reports of the author.

In conclusion it may be said that no person who must test the color vision of others should omit studying this book carefully. As Professor Bartlett said in his Foreword to the book, "There is no other book I know in which the general methodology of this type of psychophysical investigation is dealt with anything like as well."

ROBERT W. BURNHAM
Eastman Kodak Company, Rochester, New York

WERT, JAMES E., NEIDT, CHARLES O., & AHMANN, J. STANLEY. *Statistical methods in educational and psychological research.* New York: Appleton-Century-Crofts, 1954. Pp. viii+435. $5.00.

This is a lengthy but chatty introduction to descriptive and inferential statistics which progresses at a pedagogical pace suitable to the median student. Many detailed examples are well presented with emphasis on underlying rationale and assumptions. Arithmetic and high school algebra are the only requisites. The first five chapters present the customary procedures for collecting and summarizing data including central tendency, percentiles, variability, and correlation. Three chapters are then devoted to the concepts of sampling and statistical inference. The remaining 11 chapters deal with chi square, analysis of variance and covariance, linear and curvilinear regression, and elaborations of correlation. Contentwise the book's chief claim to attention in the elementary market-place is its more than ample discussion, and certain unusual material such as double classification analysis of variance with disproportionate frequencies and the application of the two-class discriminant function. A worthy contender for the first two or three semesters.

LEONARD S. KOGAN
Institute of Welfare Research, Community Service Society of New York

UNDERWOOD, BENTON J., DUNCAN, CARL P., TAYLOR, JANET A., & COTTON, JOHN W. *Elementary statistics.* New York: Appleton-Century-Crofts, 1954. Pp. ix+239. $3.25.

This little book is designed for use in the introductory undergraduate one-semester course. Coverage is classic and includes handling of distributions, percentiles, central tendency, variability, correlation, the normal curve, significance of differences between means, simple analysis of variance, and chi square. It is well written and pitched toward problems of inference in research as are a number of other elementary texts.

LEONARD S. KOGAN
Institute of Welfare Research, Community Service Society of New York

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